THE
POPULAR SCIENCE
MONTHLY.

SEPTEMBER, 1901.

THE GREATEST BIOLOGICAL STATION IN THE WORLD.*

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BIOLOGICAL, Zoological, Marine Stations are all of them merely the seaside workshops of the modern naturalist 'writ large.' But they offer wonderful facilities for the most advanced and best kinds of biological work and it is almost impossible to overestimate the influence they have had in the advancement of our knowledge of living nature. The field-naturalist of old, before the days of college laboratories, studied his animals and plants alive in the open, or collected and arranged them in his cabinets and museums. The work was interesting and necessary, but to some extent superficial. We see its importance enhanced in these later days in the light of Darwinism. It was an enormous gain to science when zoological and botanical laboratories were equipped in the universities, and when every student came to examine everything for himself and to probe as deeply as possible into structure and function. It is no wonder if for a time, in some quarters, in the fascinations of microscopic dissection and section-cutting and mounting, there was perhaps a tendency to lose sight of living nature, and to convert refinement of method and beauty of preparation into the end, in place of being only the means of the investigation.

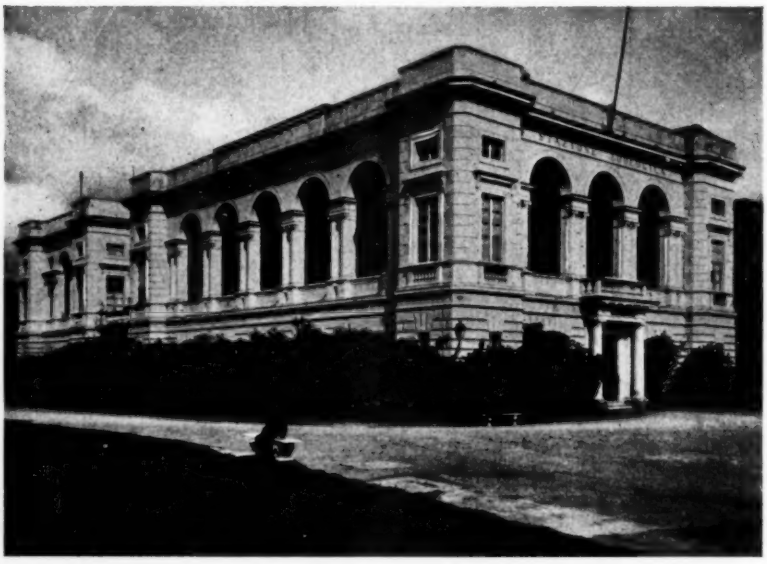
The biological station came to put all that right. It presented a happy union of the observational work of the field-naturalist with the minute investigations of the laboratory student. It brought the laboratory to the seashore, and the sea, in the form of well-equipped healthy

* From notes taken on a recent visit to the Zoological Station at Naples.

tanks, within the walls of the laboratory. It enabled the living organisms to be studied almost in their native haunts by the most refined laboratory methods.

Thirty years ago the biological station was almost unknown; now there are, I suppose, about fifty or possibly more, large and small, scattered along the shores of the civilized world from the arctic circle to the tropics and Australia, from western California to far Japan in the East—and of these the parent institution, and by far the finest and most important, is the world-renowned 'Stazione Zoologica,' under the direction of Dr. Anton Dohrn, at Naples.

It is almost impossible to think of the Naples station apart from Anton Dohrn. He is the founder, benefactor, director, the center of



THE NAPLES ZOOLOGICAL STATION.

all its activities, the source of its inspiration. He established the first building in 1872, and, although he has had support from the German and Italian Governments and from scientific institutions all over the world, still I believe it is no secret that his own private fortune used unsparingly has contributed much to the permanence and success of the undertaking. He has fostered and directed it continuously for nearly thirty years: the twenty-fifth anniversary of the foundation was celebrated on the 14th of April, 1897, by a remarkable memorial in which all the leading biologists of the world were united.

The international character of the institution is a most interesting and important feature. Situated in the south of Italy, founded and

directed by a German, subsidized (in an excellent manner described below) by most European governments, including even those of Switzerland, Hungary, Holland, Belgium and Spain, the members of the staff and the naturalists at work in the institution may be of any nation and usually are of many; and at any hour of the day at least the four languages, French, German, English and Italian, may be heard among the busy groups in the laboratory and the library.

But the Naples Zoological Station is not wholly for the scientific man—in fact many visitors to Naples do not know that science has anything to do with it. The more public department of the institution, the celebrated 'Acquario,' is one of the sights of Naples and is well known to and highly appreciated by the more intelligent of the tourists you meet at the hotels. The whole institution is usually known to the English-speaking tourist as 'The Aquarium,' and few, even of those who visit and enjoy it, seem to know or wonder anything about the remainder of the great white edifice into the ground floor alone of which they are allowed to penetrate.

The zoological station of Naples in its present condition (it was once smaller, and will probably some day soon be larger) consists of two great, white, flat-topped buildings of imposing appearance, connected by a central yard and large iron galleries, placed on the Chiaja in the Villa Nazionale, the beautiful public garden which occupies that part of the shore of the wonderful Bay of Naples. Surrounded by palms, cactus, aloes, with groups of statuary, fountains and minor temples, looking out upon the incomparable panorama from Vesuvius by Sorrento and Capri to Procida and Ischia, there is probably no finer situation in the world than that occupied by what is unquestionably the most important of zoological institutions.

As to this importance, no university laboratory approaches it. There is no other laboratory where the work-places are occupied by some forty or fifty doctors and professors and investigators of established reputation from all parts of Europe and America, who have come there to do original work, attracted by the fame of the institution and its director; no laboratory where forty such workers can be kept supplied with abundance of fresh material for their researches (of the most diverse description) brought from the sea at least twice a day; no laboratory where there are such excellent facilities for work and such charming opportunities for scientific intercourse.

The staff of the institution now consists of:

1. Professor Dr. Anton Dohrn, the founder and director.

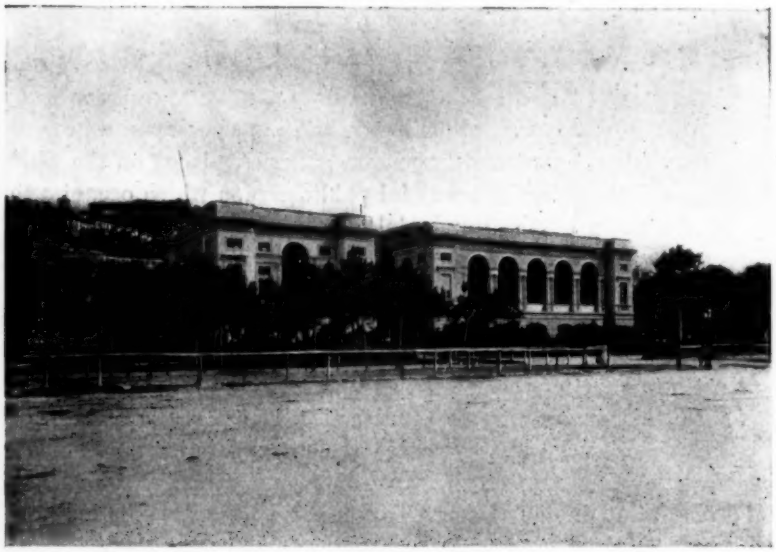
2. Seven Scientific Assistants—viz., Dr. Eisig, the administrator of the laboratories; Dr. Paul Mayer, the editor of the publications, Dr. W. Giesbrecht, the assistant editor, and the supervisor of the illustrations; Dr. Gast, also concerned in the publications in addition to other work; Dr. Schoebel, the librarian; Dr. Lo Bianco, the administrator of the fisheries and préparateur;

Dr. Hollandt, in charge of the microscopic sections department—all of them well-known men, each eminent in his own line of investigation.

The post of assistant in the physiological department formerly held by the late Dr. Schoenlein is now vacant.

3. In addition to these scientific heads of departments there are:—the secretary, Mr. Linden, two painters, and the chief engineer; and, finally, about thirty attendants, collectors and others employed in the laboratories, in the collecting and preserving departments, in the aquarium and elsewhere.

This may seem at the first thought a very large staff, but the activities of the institution are most varied and far-reaching, and everything that is undertaken is carried to a high standard of perfection. Whether it be in the exposition of living animals to the public in the wonderful tanks of the 'Acquario,' in the collection and preparation of choice specimens for museums, in the supply of laboratory material and mounted microscopic objects to universities, in the facilities afforded



THE ZOOLOGICAL STATION FROM THE EDGE OF THE SEA.

for research, or in the educational influence and inspiration which all young workers in the laboratory feel—in each and all of these directions the Naples station has a world-wide renown. And the best proof of this reputation for excellence is seen in the long list of biologists from all civilized countries who year after year obtain material from the station or enroll as workers in the laboratory. Close on 1,200 naturalists have now, since the opening of the zoological station in 1873, occupied work-tables, and, as these men have come from and gone back to practically all the important laboratories of the world, Naples may

fairly claim to have been for the last quarter-century a great international meeting ground of biologists, and so to have exercised a stimulating and coordinating influence upon biological research which it would be difficult to overestimate.

The success of the institution has caused constant additions and has stimulated the staff to fresh undertakings. To the original aquarium and zoological laboratories a second building mainly for botany and physiology and the preparation of specimens was soon added; and it is said that a third is in contemplation. In the meantime additional accommodation has been obtained during the last decade by a rearrangement of the roof of the main building. This gives space for a second large zoological laboratory, a supplementary library and various smaller rooms, used as chemical and physiological laboratories, for photography

LANDING PLACE OF THE FISHERMAN AT POSILIPO, NAPLES.

and for bacteriology. A good deal of the research in recent years, both on the part of those occupying work-tables and of the permanent staff, has been in the direction of comparative physiology, experimental embryology and the bacteriology of sea-water, and all necessary facilities for such work are now provided.

The laboratories contain accommodation for over fifty scientific men to work, and each such work-place, known technically as a 'table,' consists either of a small room or of an alcove or a portion screened off from a larger room. Such tables are rented at £100 a year, not to individuals, but to states or universities or committees, and of the fifty-five tables at present available about thirty-four are permanently engaged—thus bringing in a considerable annual subsidy to the ad-

ministration. Germany takes some ten of these tables, and Italy seven. There are, I believe, three American tables—one belonging to the Women's Association—and there are three English (rented by the Universities of Cambridge and Oxford and the British Association respectively), consequently there are generally about half-a-dozen English and American biologists at work in the station; but Dr. Dohrn interprets in a most liberal spirit the rules as to the occupancy of a table, and, as a matter of fact, during a recent visit of the present writer there were, for a short time, no less than three of us on the books as occupying simultaneously the British Association table, but in reality all provided with separate rooms.

WORK PLACES IN THE LARGE LABORATORY.

A work-table is then really a small laboratory fitted up with all that is necessary for ordinary biological research, and additional apparatus and reagents can be obtained as required. The investigator is supposed to bring his own microscope and dissecting instruments, but is supplied with alcohols, acids, stains and other chemicals, glass dishes and bottles of various kinds and sizes, drawing materials and mounting reagents. Requisition forms are placed beside the worker on which to notify his wishes in regard to material and reagents; he is visited at frequent intervals by members of the scientific staff; he has an attendant to look after his room and help in other ways, and in fact all his reasonable wants are supplied in the most perfect manner. A scientific man, or woman, then, wishing to do a special research at the Naples

station must be appointed to a particular table for a definite time by his government, university, or the controlling committee of that 'table,' and this is the system which has worked so well for over quarter of a century and which gives a certain stamp and tradition to some at least of the tables.

The opportunities for taking part in collecting expeditions at sea are most valuable to the young naturalist, and especially to such as have not had previous experience of the rich Mediterranean fauna. Dredging, 'plankton' collection and fishing are carried on daily in the Bay of Naples by means of the two little steamers (the 'Johannes Müller' and the 'Francis Balfour'—both classic names in biology) belonging to the

IN THE LIBRARY.

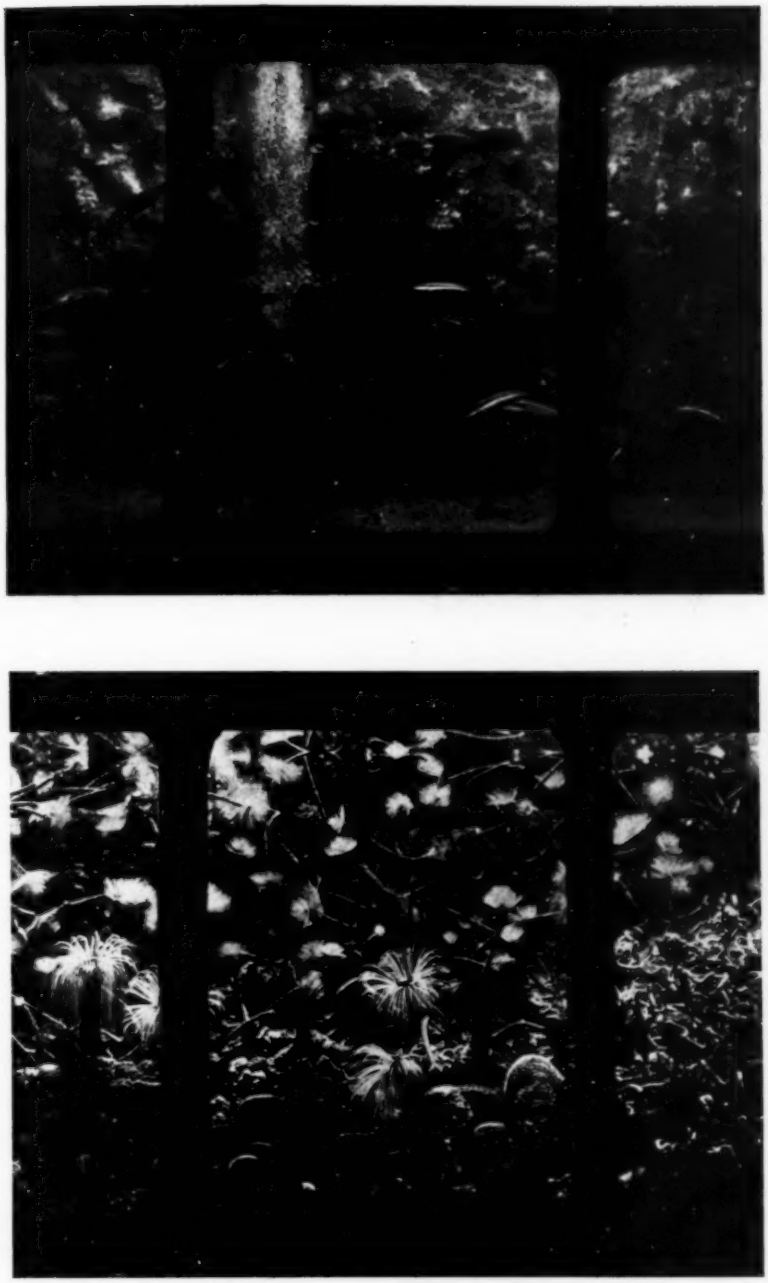
station, and by a flotilla of fishing and other smaller boats which start for work in the very early morning and return laden with treasure in time to supply the workers in the laboratory for the day. Many of the Neapolitan fishermen are more or less in the employ of the station and bring to the laboratory such rare specimens as they may chance to find in their day's work.

Chevalier Dr. S. Lo Bianco, the genial chief of the collecting and preserving department, has a phenomenal knowledge of the marine fauna, and of where, when and how to catch any particular thing—and, moreover, of how best to preserve it when caught. Each afternoon he visits the laboratories and ascertains the wants of the workers, each night he

gives his orders to his crews of fishermen, with various hints as to likely haunts and the best tactics to pursue; and the following morning sees a procession of tubs and baskets filled with glass jars, containing the specimens rich and rare, being conveyed from the little dock to the laboratory—generally balanced in wonderful piles on the heads of the stalwart and picturesque boatmen. Dredging expeditions during the day along the shores or to the neighboring bay of Pozzuoli take place in the steam launch, and workers who wish to search for some special animal or who are studying the fauna can join such trips. Then about once a fortnight or so a longer excursion is organized, say to Ischia or to Capri, occupying the whole day, and to this all in the laboratory who care for it are invited. It is on these occasions that Cav. Lo Bianco is seen—if I may say so with all respect—in his glory; directing all proceedings, the center of all activities, full of geniality and information, he is the life and soul of the party. He speaks to us in any language, and knows everything we catch on land or sea; patting the fishermen on the back, talking seriously with the strictly scientific, joking with the more versatile, sympathizing if necessary with the sea-sick and helping everyone to enjoy the day and profit by the experience, he is an ideal leader of the marine biological picnic.

The finest specimens caught or those not required for immediate investigation are either most skilfully preserved for museums or pass into the tanks of the aquarium. And it is possible, without ever going to sea, to gain a very fair idea of the local Mediterranean fauna from that last named part of the institution. The beauty and interest of the aquarium are due, of course, in great measure to the brilliancy and abundance of the rich fauna in the neighboring waters, but also in part to scientific knowledge and skill. The tanks are most carefully watched and governed, and their exact condition is always known —the temperature, specific gravity, number of bacteria present, and other particulars of the water, are constantly tested and considered. The public admiring the tanks in the ground floor little know of the 'council of war' occasionally summoned in the laboratory upstairs consisting of experts in the subjects concerned, chemistry, biology, bacteriology, to examine some unusual sample or settle some delicate question. And so, by much care and thought, results and effects are produced which we admire greatly in the aquarium and which, although no doubt in part due to the latitude, are also dependent upon the scientific knowledge and manipulative skill behind the scenes.

Amongst the fishes, we see in one tank fine specimens of the Muræna —the real old Roman eel—coiling their snake-like bodies through the necks of broken jars just as their ancestors no doubt did two thousand years ago with the same pots and jars—for those in the tanks are antiques—in the neighboring bay of Baiæ. We can see the Torpedo or



IN THE AQUARIUM.

electric ray in an open shallow tank, and by putting the thumb above and the fingers under the animal's flat shoulders, whilst we pull or squeeze the tail with the other hand, an electric shock can be obtained. Octopus, Squids and other Cuttlefish are present in abundance; crabs that mimic their surroundings, those with anemones and with sponges on their backs, animals that look like plants, corals and sea-fans of many kinds, worms that live in leathery tubes a foot long and expand out of the top, like gorgeous flowers six inches across with innumerable spirally-arranged petals—these seem to be the favorites with visitors. But probably the most interesting tanks to the scientific man are those containing the recently caught 'plankton,' the Medusæ and other delicate and gelatinous surface organisms. There is one marvelous creature that can be seen almost nowhere else, the *Cestus veneris,* which is like an undulating, pulsating band of light, in some positions absolutely transparent, in others flashing iridescent fire like a diamond from its sides. So much for the public aquarium, which, at an admission fee of two francs, brings in to the institution a revenue of about £1,000 a year. Now a word as to the publications of the station.

Workers at Naples are free to publish the results of their investigations where they like, and records of the good work in all departments of biology which has been done at this station are to be found in all civilized countries in the form of memoirs and articles contributed to the scientific periodicals of the world. But still a considerable amount of the whole, including a number of the more extended, more solid and more noteworthy contributions, has been published at Naples as a noble series of monographs on the 'Fauna and Flora of the Gulf of Naples'—each monograph being one or more quarto volumes, richly illustrated, and dealing with one particular group of animals, or a section thereof. This great series, of which 26 monographs have now appeared, is amongst the most cherished possessions of every zoological library. Besides these monographs fourteen volumes of a smaller yearly journal, the 'Mittheilungen,' have been published containing shorter but still important papers, and Dr. Paul Mayer also edits a yearly summary or record, the 'Zoologischer Jahresbericht,' of the advances made in all departments of zoology in all parts of the world.

But although the work of the Naples Zoological Station is thus many-sided, the leading idea is certainly original research. An investigator usually goes to Naples to make some particular discovery, and he goes there because he knows he will find material, facilities and environment such as exist nowhere else in the same favorable combination. As a result of the splendid pioneer work which Dr. Dohrn has done at Naples, every civilized country has now established its own biological stations, some larger, some smaller; but although these are of prime importance amongst scientific institutions, as enabling the young

investigator to commence research in living material without leaving home, it must not be thought that they detract from the advantages of a visit to the Naples station, or affect the commanding position of that unique University of Natural History. Notwithstanding Woods Holl, Heligoland and Plymouth—aye, and any others that are likely to follow—Naples is still the Mecca of the young biologist and remains the greatest biological station in the world.

H. Cavendish

HENRY CAVENDISH.

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PERHAPS the most remarkable character in the history of science is Henry Cavendish. One of the few men of science who have possessed great fortunes, he yet ignored the power of his wealth, allowing himself but few and simple wants. Highest born of the distinguished chemists of Great Britain, he cared nothing for the external advantages of birth, preferring to house himself till forty years of age in his father's stables, where unmolested he might devote his days to the pursuit of truth. Outside the monk's cell and the prisoner's dungeon, few men have lived and held so little communication with their fellows or made so few friendships as he. Yet his fame could not be kept from proclaiming itself even during his lifetime, while to-day he is called the 'Newton of Chemistry' and the 'Father of Quantitative Physics,' being declared by Biot to be 'the richest of scientists, and the most scientific of the rich.'

Of a family, tracing its pedigree to the Lord Chief Justice under Edward III., he was the son of Lord Charles Cavendish, the third son of the Duke of Devonshire, and of Lady Anne Grey, daughter of Henry, Duke of Kent, and was born October 10, 1731, at Nice, Italy, whither his mother had gone on account of ill-health. His mother died two years later, after giving birth to a second son, Frederick; and Cavendish, residing with his father in London till eleven years of age and spending the next eleven years away at school, was deprived at the most critical period of his life of the salutary influences of a happy home, that might have tempered the peculiarities of his character, which in the last analysis, however, are to be referred chiefly to original conformation.

Having entered Peterhouse College, Cambridge, in 1749, he left in 1753 without taking his degree, a step scarcely due to fear of the examinations themselves, but rather in keeping with his very pronounced shyness, which he was said to possess to a degree bordering on disease. His personal history is a blank for the next ten years, but his subsequent writings show that they were spent in mathematical, chemical and physical research. He was elected a fellow of the Royal Society in 1760.

In 1783, the death of his father left him free to follow his own tastes. During his father's lifetime he was kept on an annuity of

£500, and some regard this fact as explaining in part the peculiarities of his character; for during this period he acquired those habits of economy and those singular oddities of character which he ever afterward exhibited in so striking a manner. For some years Cavendish was allowed by his father to attend the Royal Society Club regularly, but was given the exact five shillings for the dinner, not a penny more. There is reason to believe that his father's parsimony has been misapprehended, for while Cuvier, Biot and Lord Brougham make dissatisfaction with Henry for not entering on public or political life the ground for his illiberality towards him, yet others assert that Lord Charles Cavendish was not a rich man and allowed his son all he could afford. There is no certainty as to when or from what source Cavendish inherited the riches which ultimately came into his possession, though they were probably a legacy from a rich uncle. All the testimony, however, is at one on two cardinal facts: that Cavendish was for the first forty years of his life a poor man, and for the last thirty-nine an exceedingly wealthy one.

The possession of several hundred thousand pounds did not alter his life in the least; he simply did not know what to do with it, and hence let it alone, allowing it to accumulate till at his death his estate was worth £1,175,000. Cavendish's indifference to pecuniary affairs was so great that when his banker called on him with regard to the investment of a portion of the vast sum that had grown on his hands, he was rudely ordered to be gone and not to come there to plague him, or he would lose the control of the funds. It may seem strange that none of this large fortune was devoted to scientific or charitable purposes, but we must remember that Cavendish never thought of himself, much less of others. Sir Humphry Davy was indebted to him for 'some bits of platinum,' but tacitly appealed in vain for financial aid in his electrical researches. Just before the subscription for the enlarged voltaic battery was taken, Cavendish was in Davy's apartments at the Royal Institution, and upon Davy expressing fear that he should fail to secure the necessary amount, Cavendish joined heartily in deploring the lack of liberality in the patrons of science, but did not seem to consider himself at all called upon actively to forward the desired object. Yet had he been directly asked to sign a cheque in Sir Humphry's name for £500 he would probably have done so at once. When reminded of some needy charitable object, he gave liberally, but he never himself saw a need. Whether from original or acquired indifference, he exhibited a *passive* selfishness in all his dealings.

To his town residence, close to the British Museum, few visitors were admitted, and these have reported it to contain only books and apparatus. For the former he also set aside a separate mansion on Bradford Square, and here collected a large and carefully chosen library

of science, which he threw open to all engaged in research, and from which he himself never took a book without leaving a formal receipt. His favorite residence was a beautiful suburban villa at Clapham, which now, as well as a street in the neighborhood, bears his name. The whole house was occupied with workshops and laboratory, only a small part being set aside for personal comfort. He needed nothing more for himself, and he did not wish others to visit him. When occasionally he had guests, they were always feasted on a leg of mutton. On one occasion his housekeeper suggested that a leg of mutton would not be enough. Well, then get two, was the reply.

The more prominent of Cavendish's contemporaries have left graphic estimates of his remarkable and interesting peculiarities of character. The most striking was a singular love of being alone. He held aloof from social intercourse, even with members of his own family, and only once a year saw the one he had made his heir. To the great objects of common regard which excite the fancy, the emotions and the higher affections, he was equally indifferent. The beautiful, the sublime and the spiritual seem to have lain entirely beyond his horizon. Although he is thought to have held Unitarian views, he is also understood never to have attended a place of worship. In the words of one of his contemporaries, 'he was the coldest and most indifferent of mortals.' He never married and was reputed to have a positive dislike for women. Lord Brougham tells us that Cavendish ordered his dinner daily by a note, left on the hall table, where the housekeeper could afterwards get it. Another authority relates that Cavendish 'one day met a maid servant on the stairs with a broom and pail, and was so annoyed that he immediately ordered a back staircase to be built.' His dress was that of the gentleman of the preceding half century. The frilled shirt-waist, the greatcoat of a greenish color, and the cocked hat, made a picture no one was likely to mistake. But gleams of genius often broke through this unpromising exterior. He never spoke except to the point, and always supplied excellent information or drew some important conclusion from his own very extensive and accurate knowledge. So that while Sir Humphry Davy said of him, "His voice was squeaking, his manner nervous, he was afraid of strangers, and seemed, when embarrassed, even to articulate with difficulty," he also said, "He was acute, sagacious, and profound, and, I think, the most accomplished British philosopher of his time."

But two additonal dates remain to be given in reference to his personal history: The first, March 25, 1803, when he was elected one of the eight foreign associates of the French Institute; the second, February 24, 1810, when he died, in his seventy-ninth year. As he lived so he died by rule, predicting his death as if it had been the eclipse of some great luminary (as indeed it was), and counting the moment when

the shadow of the unseen world should enshroud him in its darkness. After an illness of only three days, the only one he ever had, he called his servant, told him he was going to die, and commanded him to stay away and to keep everyone else away until the event was over. The servant obeyed, and, when he returned, Cavendish had breathed his last.

An intellectual head thinking, a pair of wonderfully acute eyes observing, and a pair of very skilful hands experimenting and recording, are all that we realize in reading his memorials. His theory of the universe seems to have been that it consisted of a multitude of objects to be weighed, numbered and measured; and the vocation to which he thought himself called was to weigh, number and measure as many of these objects as his threescore years would allow. Whenever we catch sight of him, we find him with his measuring rod and balance, his graduated jar, thermometer, barometer and table of logarithms. Most of his researches were avowedly quantitative; he weighed the earth, he analyzed the air, he discovered the compound nature of water, and he noted with numerical precision the actions of the ancient element fire. Everything pertaining to each, to which a quantitative value could be attached, was set down in figures, before it went out to the scientific world with its passport signed and sealed. In all his researches he displayed the greatest caution, not from hesitation or timidity, but from his recognition of the difficulties which attend the investigation of nature. *Cavendo tutus* was the motto of his family, and seems ever to have been before him, so that he well deserves the title—'Father of Quantitative Physics.'

His first recorded scientific work was 'Experiments on Arsenic' (1764). In 1765 'Experiments in Heat' were performed which, though written out for a friend, were not made public till nineteen years later, but which, had they been published in 1764, would have given Cavendish precedence to Black in some of his discoveries as to 'latent heat' and 'specific heat,' and equal merit in others. In his first public contribution to science, 'Experiments on Factitious Airs,' sent to the Royal Society in 1766, he defines 'factitious air' as air which is driven off when compounds are heated or treated with acids, and the questions of the permanent elasticity of 'factitious airs,' their solubility in different liquids, their power to support combustion, their specific gravity, and likewise their combining equivalents, were all carefully considered. 'Fixed air' (CO_2) was only a particular kind of factitious air driven off from the alkalis (carbonates), and he found that when it was mixed with common air in the proportion of one part to nine, it rendered the air unfit for respiration. Cavendish first isolated and experimented with hydrogen, though he cannot be called its discoverer, for Paracelsus, about 1540, obtained it by acting on metals

with sulphuric acid, the result of which he described as the 'rising of the wind'; and many of Cavendish's predecessors, Boyle among others, had encountered it; but Cavendish, who called the gas 'inflammable air,' was the first to examine its properties carefully and to describe them.* Cavendish is also entitled to be called the discoverer of the constant composition of the atmosphere, and its first accurate analyst, for in 'An Account of a New Eudiometer' (1783) he showed the atmosphere to be of constant composition and to consist chiefly of 'phlogisticated' and 'dephlogisticated air' (nitrogen and oxygen), and he observed that when the electric spark passing through his eudiometer caused the 'phlogisticated' and 'dephlogisticated air' to unite, there was always left a small bubble which he could not get rid of in any way. This small bubble we now know to have been *'argon.'* In his celebrated paper read before the Royal Society in 1784, on 'Experiments on Air,' he gave an account of the discovery of the composition of water and of nitric acid. He showed that nitric acid, which had been known by Geber probably in the eighth century, was produced when nitrogen mixed in small quantity with hydrogen was exploded by the electric spark in the presence of an excess of oxygen. But strictly speaking we cannot assign to him the merit of the discovery of the composition of nitric acid, for he regarded nitric acid as a simple, or at least an undecompounded body, while nitrogen, according to him, was a compound. He was thus not the direct asserter of the modern doctrine of the composition of nitric acid, and to Lavoisier belongs the merit of the true interpretation of Cavendish's results.

Wilson's presentation of 'A Critical Inquiry into the Claims of All the Alleged Authors of the Discovery of the Composition of Water'† makes it certain that Cavendish was the first consciously to convert hydrogen and oxygen into water, and to teach that it consisted of them. In his own words 'water consists of dephlogisticated air united with phlogiston,' and as dephlogisticated air was his term for oxygen and phlogiston his term for hydrogen, this statement corresponds closely with the modern view of the nature of water. His inheritance of the prejudices of the early phlogiston school led him to the erroneous conclusion that every combustible contains hydrogen, and that the deoxidation of air and the oxidation of combustibles are invariably accompanied by the production of water. The discoverer of so great a truth as the composition of water may be forgiven for overestimating its importance.

While his experiments on the composition of water were made in the summer of 1781, his paper, 'Experiments on Air,' was not read

* Lavoisier named the gas 'hydrogen,' i. e., water-former.

† In the 'Life and Works of Cavendish,' by Dr. G. Wilson, published for the Cavendish Society, London, in 1851.

to the Royal Society till January, 1784; and this delay, resulting from his desire to investigate the nature of the acid (nitric) formed on the passage of the electric spark through a mixture of hydrogen and oxygen, containing, as was afterwards found, a little nitrogen, caused his claim to the discovery of the composition of water to be contested by no less rivals than the celebrated James Watt and the great French chemist, Lavoisier. The modest, retiring and almost inordinately cautious man, whose personal history has just been detailed, has been accused of both incapacity and dishonesty, by men distinguished in letters and science, whose connection with the vexed question gives it an interest apart from its intrinsic merit.

Though Cuvier, in 1812, as secretary of the French Academy in reading an *éloge* on Cavendish could say that "his demeanor and the modest tone of his writings procured him the uncommon distinction of never having his repose disturbed either by jealousy or by criticism," Cuvier's distinguished successor, Arago, in writing the éloge on Watt in 1839, charged that Cavendish learned the composition of water, not by experiments of his own, but by obtaining sight of a letter from Watt to Priestley. The French Academy heard the one side argued, and the British Association in the same year heard Cavendish's vindication delivered by the Rev. W. Vernon Harcourt, the president for that year.

At the very threshold of the water controversy we encounter a perplexing dilemma. Two unusually modest and unambitious men, universally respected for their integrity, famous for their discoveries and inventions, and possessed of rare intelligence, are suddenly found standing in a hostile relation to each other, and, although declining to publish their own unquestioned achievements, are seen contending for a single discovery, which the one believes the other to have learned at second hand from the revelations made to a common friend, and which that other accuses his rival of having gathered from a letter that he was allowed to peruse. A misunderstanding such as this would never have occurred had Watt and Cavendish been intimate in 1783. As yet, however, the friendly intercourse which afterwards subsisted between them had not commenced. The one was resident in London, the other in Birmingham, and each was informed of the other's doings by third parties, upon whom mainly though not equally, rests the blame of having occasioned the water controversy. Those in question are: Dr. Priestley, J. A. DeLuc and Sir Charles Blagden, all eminent men of unblemished character. Through the first, knowledge of Cavendish's experiments passed to Watt, and a knowledge of Watt's conclusions to Cavendish; by the second, Watt was informed that Cavendish had deliberately pilfered his theory; and the third, who was Cavendish's assistant, reported the latter's conclusions as well as those of Watt, to Lavoisier, whom he accused of appropriating the ideas of both English

philosophers. Blagden also made certain changes in the manuscript of Cavendish's 'Experiments on Air,' and while superintending, in his capacity of secretary of the Royal Society, the printing of the paper, and of Watt's rival essay, suffered certain typographical errors to occur, which involved himself and his principle in accusations of unfairness, in which, however, Wilson shows that with the exception of a carelessness in correcting printer's proof, Blagden was guiltless of any wrong toward Watt or of unfairness toward Lavoisier, and that to DeLuc belongs the unenviable distinction of deliberately provoking the water controversy, doing Cavendish and also Watt a great wrong by hastily deciding against the former, and filling Watt's mind with suspicions that Cavendish had borrowed from Watt's letter to Priestley the views which he published as his own, because he had in truth discovered them for himself, and that too at an earlier date (1781) than Watt (1784) or Lavoisier (1784-86).

Remarkable as Cavendish's achievements in chemistry were, his greatest fame as a scientist will ever be based on his researches in physics; his single experiment in dynamics would place him in the first rank, and a very large part of the modern development of electrical science is based upon his work in electrostatics and electro-dynamics.

Cavendish verified the law of inverse squares for gravitational attraction, and determined the mean density of the Earth. The apparatus, devised by the Rev. John Michell, consisted of a horizontal lever six feet long, suspended by a wire forty inches in length, carrying at each end a lead ball two inches in diameter. Two large masses of lead (each about 317 lbs.) were placed near the ends of the lever and on opposite sides, so that their attraction would produce turning moments in the same direction. The angle of rotation having been measured by means of telescopes with verniers, and the torsion of the suspending wire determined by observing the time of vibration of the rod, the force was calculated which would have been exerted by a globe of water the size of the earth on the same body on its surface, and from this the density of the earth was obtained as the mean of seventeen terminations to be 5.48 ± 0.38.*

Cavendish cared more for investigation than for publication. He would undertake the most laborious researches in order to clear up a difficulty which none but himself could apprehend, or was even aware of, and we cannot doubt that the result of his enquiries gave him a certain degree of satisfaction. But it did not excite in him the desire to communicate the discovery to others which, in the case of ordinary men of science, generally ensures the publication of their results. How

* This work has recently been edited, together with that of Newton, Bouguer and others, by Professor A. S. Mackenzie in 'The Laws of Gravitation,' Scientific Memoirs, American Book Company.

completely these researches of Cavendish remained unknown to other men of science is shown by the subsequent external history of electricity.

Cavendish's work as a member of the committee appointed by the Royal Society to investigate protection from lightning shows him cooperating with Franklin and others in an investigation on behalf of the nation. But most of his work was a private matter and in electrical science, in which he was by far *the* authority of his day, he published only two papers, 'Of the Electrical Property of the Torpedo' (1776) and 'An Attempt to explain some of the principal Phenomena of Electricity by means of an Elastic Fluid' (1771-72). Yet he left behind him some twenty packets of manuscript on mathematical and experimental electricity, which were but little known till Maxwell edited them in 1879, for they were only alluded to in his celebrated paper on the Torpedo. They anticipated, however, many of the facts subsequently made known by Coulomb and other celebrated physicists, and contained some of the results of experiments of a refined kind instituted at a much later day.

Cavendish proved the law of inverse squares for electric charges not by actually measuring the forces as in the case of gravitational attraction, but by showing that the entire charge resides on the surface of a charged body and that there is no charge at all communicated to a sphere within a sphere when electrically connected and a positive charge is given to the outer one. He then established the theorem that the force must vary inversely as the second power of the distance between charges in order to explain this result of experiment, showing that if it varied according to any higher power, the inner globe would receive a part of the positive charge, if according to any lower power, the inner globe would be negatively charged.

These experiments on the law of inverse squares were performed in December, 1772, and in fact all of his work in electrostatics was completed before 1774, while it was not till 1785 that Coulomb published the first of his seven memoirs, on the data of which the mathematical theory of electricity as we now know it was founded by Poisson; and as Cavendish never published his at all, it is plain that each worked in ignorance of the other's results. The method of each was distinctly his own. Coulomb made direct measurements of the electric force at different distances and compared the density of the surface charge on different parts of conductors. On the other hand, the very idea of the capacity of a conductor as a subject for investigation is due entirely to Cavendish, and nothing equivalent to it occurs in the memoirs of Coulomb. The method that Cavendish adhered to throughout his experimental work was the comparison of capacities, and the formation of a graduated series of condensers, such as is now regarded as the most important apparatus in electrostatic measurements.

Great difficulty is very often encountered in interpreting the work of former experimenters in terms of modern units, yet Cavendish had such a clear insight and worked so quantitatively that we can readily express his results in terms of modern nomenclature and units. His 'inches of electricity,' for instance, can be directly compared with modern measurements, for while his 'inches' express the diameter of a sphere of equal capacity, modern measurements express capacity as the radius of the same sphere in centimeters. When we consider the crudeness of some of Cavendish's apparatus, we are amazed at the accuracy of the results he obtained. The capacity of a circular disc, for example, was determined experimentally by him to be 1/1.57 of that of a sphere of the same radius, while the most modern calculation gives 1/1.571 for the same ratio.

Cavendish also entertained exceedingly clear views on what we now know as 'potential' and 'resistance,' and, besides Coulomb's law of inverse squares, his papers contain anticipations of Faraday's 'specific inductive capacity' and 'electric absorption,' and Ohm's 'law of electrical resistance.' In observing that the charges of coated plates were always several times greater than the charges computed from their thickness and the area of the coatings, Cavendish not only anticipated Faraday's discovery of the specific inductive capacity of different substances, but actually measured its numerical value in some cases. He also considered the question, of fundamental importance in the theory of dielectrics, whether the electric induction is strictly proportional to the electromotive force which produces it, or in other words is the capacity of a condenser the same for high as for low potentials. He regarded his results as not decisive, but, in observing that the apparent capacity of a Florence flask was greater when it continued charged a good while than when it was charged and discharged immediately, Cavendish discovered the phenomenon called by Faraday 'electric absorption,' which was fully studied later for different kinds of glass by Dr. Hopkinson, and connected with the long-known phenomenon of 'residual charge.'

But besides this series of experiments on electric capacity, another course of experiments on electric resistance was going on between 1773 and 1781, the knowledge of which seems never to have been communicated to the world till Maxwell edited Cavendish's electrical researches in 1879. We learn from the manuscripts thus published that Cavendish was his own galvanometer, comparing the intensity of currents by the intensity of the sensations he felt in his wrist and elbows when they passed through his body. The accuracy of his discriminations of the intensity of shocks is truly marvelous, whether we judge by the consistency of his results among themselves or by comparing them with the latest results obtained with a galvanometer, using all

the precautions suggested by experience in measuring the resistance of electrolytes. Indeed, such was the reputation of Cavendish for scientific accuracy, that his bare results were accepted at once and readily became a part of general knowledge, although no one conjectured by what method he had obtained them, more than forty years before the invention of the galvanometer, the only instrument by which any one else has ever been able to compare electrical resistances. In carrying out this work Cavendish not only arrived at the result, which Kohlrausch has since shown to be nearly accurate, that for weak solutions the product of the resistance by the percentage of salt is nearly constant, and also stated accurately the laws of multiple and divided currents, but he even anticipated, in January, 1781, the law of electrical resistance, discovered independently by Ohm and published by him in 1827. Moreover, in a very remarkable set of experiments on a series of salts and acids in order to determine their relative electric resistance, Cavendish tells us, 'that the quantity of acid in each should be equivalent to that in a solution of salt in twenty-nine of water,' and it is difficult to account for agreement not only of the ratios, but also for the absolute numbers given by Cavendish with those of the modern system, in which the equivalent weight of hydrogen is taken as unity. They must have been derived from his own work, for Wenzel's 'Lehre von der Verwandschaften' was published in 1777, and also gives values greater than those used by Cavendish, and Richter's 'Anfangsgrunde der Stochyometrie' was not published till 1792, while Cavendish's experiments were made in 1777. It is only by comparing the dates of these researches with the dates of the principal discoveries in chemistry that we become aware that in the incidental mention of these numbers we have the sole record of one of those secret and solitary researches, the value of which to other men of science Cavendish does not seem to have taken into account after he had satisfied his own mind as to the facts. He dealt with his discoveries as with his great wealth, allowing the larger part of them to lie unused.

A STUDY OF BRITISH GENIUS.

By HAVELOCK ELLIS.

SUMMARY AND CONCLUSIONS.

WE have now examined all those characteristics of the most eminent British persons of intellectual ability which the 'Dictionary of National Biography' enables us to investigate in a fairly generalized manner. We have found that, excluding the living, at least 902 persons (859 men and 43 women) of such preeminent ability have appeared in the British Islands between the fourth and the end of the nineteenth centuries, the century richest in genius being, so far as we can trace, the eighteenth. We have found that, in regard to distribution among the various elements of nationality, England seems to have her fair proportion of eminent persons, Scotland an excess, Ireland and Wales a deficiency, though Ireland and Wales profit considerably among those cases in which there has been intermixture; the only important foreign strain is derived from France. We have found that, as regards social class, the upper and upper middle classes have been peculiarly rich in genius, that the country and small towns have chiefly yielded notable men, and that of all professions the clergy have produced by far the greatest number of distinguished children. Our inquiry, further, confirms the views of Galton and others that intellectual ability is to some extent hereditary, though it may well be that different kinds of ability are not all equally apt to be transmissible. We have found that persons of genius, like the members of other mentally abnormal groups, tend to belong to unusually large families, are much oftener youngest children, and still more eldest children, than in any intermediate position, and that, much more frequently than in the case of the ordinary population, they are the offspring of elderly parents. These eminent persons, we have seen, have in a notable number of instances showed remarkably feeble health during infancy and childhood (being in many cases the only surviving children of large families) but have tended to become more robust as they grew older, and they have been notably precocious. Though not generally subjected to very strenuous intellectual training, they have usually enjoyed excellent opportunities for intellectual development; the majority were at some university; a very large proportion possessed extended opportunities for studying life in foreign lands during youth or early manhood. There is a marked tendency to a celibate life, and marriage when it

occurs tends to take place rather late; there is an excess of sterile marriages, though the fertility of the fertile marriages, while below that of the parents of the eminent persons, does not appear to be small when compared with the general population. We have seen that the longevity of our intellectual persons is great; we have also seen that they show a special liability to suffer from nervous affections like *angina pectoris* and asthma, while gout is peculiarly frequent; insanity is also unusually frequent. Minor mental anomalies, like stammering, are remarkably prevalent. There is also a tendency to melancholy. These are the chief conclusions we have reached concerning British persons of intellectual ability.

It may be reasonable to ask how far these are the characteristics of British persons of genius, and to what degree an investigation of persons of eminent intellectual aptitude belonging to other countries would bring out different results. It is not possible to answer this question quite decisively. The fact, however, that at many points our investigation simply gives precision to characteristics which have been noted as marking genius in various countries seems to indicate that in all probability the characters that constitute genius are fundamentally alike in all countries, though it may well be that minor modifications are associated with national differences. The point is one that can only be decisively settled when similar investigations are carried out concerning similar groups of persons of superior intellectual ability belonging to various countries.

A further question may be asked. How far has confusion been introduced by lumping together persons whose intellectual aptitudes have been shown in very different fields? May not the average biological characteristics of the man of science be the reverse of those of the actor, and those of the divine at the other extreme from those of the lawyer? I believe that Galton is inclined to assume that the investigation of groups of men with different intellectual aptitudes would yield different results. As, however, we have seen, the investigation of eminent British persons, when carried out without reference to the particular fields in which their activity has been exercised, yields results which, when comparable with those of Galton, do not usually show any striking discrepancies. Nor, so far as I have at present looked into the matter, does it appear that on the whole, when we consider separately the various groups of British eminent persons we are here concerned with, such groups show any widely varying biological characters. Certain variations there certainly are; we have seen that the geographical distribution of the various departments of intellectual activity to some extent varies, and also that in pigmentation there are in some cases marked variations. On the whole, however, it would appear that, whatever the field in which it displays itself, the elements that con-

stitute the temperament of genius show a tendency to resemble each other.

I shall probably be asked to define precisely what the 'temperament' is that underlies genius. That, however, is a question which the material before us only enables us to approach very cautiously. There are two distinct tendencies among writers on genius. On the one hand are those who seem to assume that genius is a strictly normal variation. This is the standpoint of Galton.* On the other hand are those, chiefly alienists, who assume that genius is fundamentally a pathological condition and closely allied to insanity. This is the position of Lombroso, who compares genius to a pearl,—so regarding it as a pathological condition, the result of morbid irritation, which by chance has produced a beautiful result,—and who seeks to find the germs of genius among the literary and artistic productions of the inmates of lunatic asylums.

It can scarcely be said that the course of our investigation, uncertain as it may sometimes appear, has led to either of these conclusions. On the one hand, we have found along various lines the marked prevalence of conditions which can scarcely be said to be consonant with a normal degree of health or the normal conditions of vitality; on the other hand, it cannot be said that we have seen any ground to infer that there is any general connection between genius and insanity, or that genius tends to proceed from families in which insanity is prevalent; for while it is certainly true that insanity occurs with remarkable frequency among men of genius, it is very rare to find that periods of intellectual ability are combined with periods of insanity, and it is, moreover, notable that (putting aside senile forms of insanity) the intellectual achievements of those eminent men in whom unquestionable insanity has occurred have rarely been of a very high order. We cannot, therefore, regard genius either as a purely healthy variation occurring within normal limits nor yet as a radically pathological condition, not even as an alternation—a sort of allotropic form—of insanity. We may rather regard it as a very highly sensitive and complexly developed adjustment of the nervous system along special lines, with concomitant tendency to defect along other lines. Its elaborate organization along special lines is built up on a basis even less highly organized than that of the ordinary average man. It is no paradox to say that the real affinity of genius—and I am now speaking of the highest manifestations of human intellect, of genius in so far as it can be distinguished from talent—is with congenital imbecility rather than with insanity. If indeed we consider the matter well we see that it must be so. The organization that is well adapted for adjust-

* In the preface to the second edition of 'Hereditary Genius' Galton has somewhat modified this view.

ment to the ordinary activities of the life it is born into is not prompted to find new adjustments to suit itself. The organic inhibition of ordinary activities is, necessarily, a highly favorable condition for the development of extraordinary abilities, when these are present in a latent condition. Hence it is that so many men of the highest intellectual aptitudes have so often shown the tendency to muscular incoordination and clumsiness which marks idiots, and that even within the intellectual sphere, when straying outside their own province, they have frequently shown a lack of perception which placed them on scarcely so high a level as the man of average intelligence. It is not surprising that by means of the *idiots savants,* the wonderful calculators, the mattoids and 'men of one idea,' and the men whose intellectual originality is strictly confined to one field, we may bridge the gulf that divides idiocy from genius.

Since a basis of organic inaptitude—a condition which in a more marked and unmitigated form we call imbecility—may thus often be traced at the foundation of genius, we must regard it as a more fundamental fact in the constitution of genius than the undue prevalence of insanity, which is merely a state of mental dissolution, in nearly every case temporarily or permanently abolishing the aptitude for intellectual achievement. But it must not, therefore, be hastily concluded that the prevalence of insanity among men of genius is an accidental fact, meaningless or unaccountable. In reality it is a very significant fact. The intense cerebral energy of intellectual creation involves an expenditure of tissue which is not the dissolution of insanity, for waste and repair must here be balanced, but it reveals an instability which may sink into the mere dissolution of insanity, if the balance of waste and repair is lost and the high pressure tension falls out of gear. Insanity is rather a Nemesis of the peculiar intellectual energy of genius exerted at a prolonged high tension than an essential element in the foundation of genius. But a germinal nervous instability, such as to the ordinary mind simulates some form of insanity, is certainly present from the first in many cases of genius and is certainly of immense value in creating the visions or stimulating the productiveness of men of genius. We have seen how significant a gouty inheritance seems to be. A typical example of this in recent years was presented by William Morris, a man of very original genius, of great physical vigor and strength, of immense capacity for work, who was at the same time abnormally restless, very irritable, and liable to random explosions of nervous energy. Morris inherited from his mother's side a peculiarly strong and solid constitution; on his father's side he inherited a neurotic and gouty strain. It is evident that, given the robust constitution, the germinal instability furnished by such a morbid element as this—falling far short of insanity—acts as a precious fermentative ele-

ment, an essential constituent in the man's genius. The mistake usually made is to exaggerate the insane character of such a fermentative element, and at the same time to ignore the element of sane and robust vigor which is equally essential to any high degree of genius. We may perhaps accept the ancient dictum of Aristotle as reported by Seneca: 'No great genius without some mixture of insanity.' But we have to remember that the 'insanity' is not more than a mixture, and it must be a finely tempered mixture.

This conclusion, suggested by our survey of British persons of preeminent intellectual aptitude, is thus by no means either novel or modern. It is that of most cautious and sagacious inquirers. The same position was, rather vaguely, adopted by Moreau (de Tours) in his *Psychologie morbide dans ses rapports,* etc., published in 1859, though, as his book was prolix and badly written, his proposition has often been misunderstood. He regarded genius as a 'neurosis,' but he looked upon such 'névrose' as simply "the synonym of exaltation (I do not say trouble or perturbation) of the intellectual faculties, . . . The word 'neurosis' would indicate a particular disposition of the faculties, a disposition still in part physiological, but overflowing these physiological limits"; and he presents a genealogical tree with genius, insanity, crime, etc., among its branches; the common root being 'the hereditary idiosyncratic nervous state.' J. Grasset, again, more recently (*La supériorité intellectuelle et la névrose,* 1900), while not regarding genius as a neurosis, considers that it is united to the neuroses by a common trunk, this trunk being a temperament and not a disease. The slight admixture of morbidity penetrating an otherwise healthy constitution, such as the present investigation suggests as of frequent occurrence in genius, results in an organization marked by what Moreau calls a 'neurosis' and Grasset a 'temperament.'

It has been necessary to state, as clearly as may be possible, the conclusions suggested by the present study as regards the pathological relationships of genius, because, although those conclusions are not essentially novel, the question is one that is apt to call out extravagant answers in one direction or another. The most fruitful part of our investigation seems, however, to lie not in the aid it may give towards the exact definition of genius—for which our knowledge is not sufficient—but in the promising fields it seems to open out for the analysis of genius along definite and precise lines. The time has gone by for the vague and general discussion of genius. We are likely to learn much more about its causation and nature by following out a number of detailed lines of inquiry on a carefully objective basis. Such an inquiry, as we have seen, is difficult on account of the defective nature of the material and the lack of adequate normal standards of comparison. Yet even with these limita-

tions it has not been wholly unprofitable. It has enabled us to trace a number of conditions which, even if they cannot always be described as factors of the genius constitution, clearly appear among the influences highly favorable to its development. Such a condition seems to be the great reproductive activity of the parents, the child destined to attain intellectual eminence in many cases alone surviving. The fact of being either the youngest or the eldest child is a condition favorable for subsequent intellectual eminence; and I may add that I could refer to numerous recent instances of large families, in which the eldest and the youngest, but no other members, have attained intellectual distinction. We have further seen that there is a tendency for children who develop genius to be of feeble health, or otherwise disabled, during the period of physical development. It is easy to see the significance of this influence which by its unfavorable effects on the development of the limbs—an effect not exerted on the head which may thus remain relatively large—leaves an unusual surplus of energy to be used in other directions; at the same time the child, who is thus deprived of the ordinary occupations of childhood, is thrown back on to more solitary and more intellectual pursuits. The clumsiness and other muscular incoordinations which we have found to be prevalent—while there is good reason to believe that they are of congenital origin—cooperate to the same end. Again, it is easy to see how the shock of contact with a strange and novel environment, which we have proved to be so frequent, acts as a most powerful stimulant to the nascent intellectual aptitudes. It is possible to take a number of other common peculiarities in the course of the development of genius and to show how they either serve to inhibit the growth of genius along unfruitful lines or to further it along fruitful lines.

Such an investigation as the present is far from enabling us to state definitely all the determining factors of genius, or even all the conditions required for its development. It suggests that they are really very numerous and that genius is the happy result of a combination of many concomitant circumstances, though some of the prenatal group of circumstances must remain largely outside our ken. We are entitled to believe that the factors of genius include the nature of the various stocks meeting together in the individual and the manner of their combination, the avocations of the parents, the circumstances attending conception, pregnancy and birth, the early environment and all the manifold influences to which the child is subjected from infancy to youth. The precise weight and value of these manifold circumstances in the production of genius it must be left to later investigators to determine.

THE STATISTICAL STUDY OF EVOLUTION.

By Professor C. B. DAVENPORT,
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AS I was going through the chemical section of the John Crerar Library the other day I stopped to examine two books. The first was one of those dawning-chemical works—Glauber's—which enjoyed a wide reputation in the sixteenth and seventeenth centuries. It contained an abundance of speculation and of recipes; but the recipes were of a purely qualitative sort—mix this and this, the *amount* is immaterial. The second book was the lectures of Van't Hoff, marking the most recent development of the science of chemistry. This book is full of formulae and tables; numbers, signs and quantities fill every page; they are symbols not only of quantitative relations but also of the direction and cause of advance of chemistry since the days of the alchemists. The history of chemistry is the history of the other quantitative natural sciences, of astronomy and physics. As science advances its methods become more and more quantitative.

Biology is often contrasted with physics and chemistry as a qualitative science. But there is nothing so fundamentally dissimilar in the phenomena of chemistry and biology that they must necessarily be studied so differently. Both treat of matter, not only statically but also dynamically. But the phenomena of biology are more complex than those of chemistry, the things to be described and compared are more numerous; and so the science, which is hardly more than a century old, is still in the descriptive and comparative stage. But the history of science in general justifies the prediction that biology, too, will in time use chiefly quantitative methods in studying processes.

Evolution is an organic process. It has been studied in various ways. Many have sought by ingenious logic to discover its workings. Others have reasoned by analogy from the effects of artificial breeding. Others still, having observed a probable factor of evolution in one case, have argued for its universality. But all of these methods have been qualitative. More recently, on the other hand, there has been undertaken an exact, quantitative study of the condition of species in nature under different environments, to determine exactly the effects that the different environments have produced. This new method, which now demands our attention, is nothing less than the quantitative study of evolution.

We all know that a lot of people, taken at random, show individual physical differences. Even when the people form a homogeneous lot and are of one sex, are all adults, and are of one race, and when such individuals as are very pathological or freaks are excluded, we still find that they differ in stature, weight, proportions of trunk and appendages, color of hair and eyes, proportions of facial features and various other characters. These differences are so slight and multitudinous that the language of adjectives fails to indicate them; measurements must be made in order that they may be expressed numerically.

But how can these numbers tell us anything about the evolution of the human species? The general principle is this: The differences between the races of man are of the same kind as, and differ only in degree from the differences between the individuals of a race. Consequently, the laws of individual variation in a race may be relied on to illuminate the method of origin of the race or species under consideration. Just how, will be clearer after we have considered the sorts of individual variation.

The laws of nature are got at only with the key of the proper method. And it is only within recent years that an adequate quantitative method has been developed in biology. It will now be necessary to consider this method.

Imagine a file of 40 men arranged in order of stature—the tallest at the head of the column. The crown of their heads will form a flowing curve, nearly level in the middle of its course and becoming more oblique at the ends. The reason for this is that the middle statures are much more common than the extreme ones. Half the people have a stature within two inches of the average (Fig. 1). The general features of such a curve are common to all classes of men, but the details differ in different classes of men. The characteristic differences are measured by what are called the *constants* of the curve.

The stature of the middle man in the file will give us very nearly the first constant, the mean or average. The average is obtained exactly by adding all the individual statures together and dividing by the number of men (*e. g.*, by 40). The average is used constantly and as a matter of course by nearly every one who wishes to compare two comparable series of numbers. It is awkward to compare all the separate data so we let the average stand for the lot. The average is, however, an entirely ideal quantity which need not agree with the measurement of any individual; and it is a little curious that it is so universally used in statistics. Of a series of measurements made on one and the same dimension, the average is demonstrably nearest the true value, and consequently engineers, physicists, astronomers and others who aim at the greatest possible precision in the measurement of the individual

phenomenon must make use of it. But in measurements made on a series of distinct individuals the average does not signify the value nearest the truth, and we cannot infer that it is the most significant single representative of the series. The average has this disadvantage, moreover, that the introduction of a few very extraordinary individuals has undue influence on the result. Thus in calculating the average income of American colleges, one institution with an income of $1,200,000 increases the average by an amount ($2,500) equal to the total income of about 5 per cent. of the 'colleges and universities.' The average has indeed been over-rated and over-used, as though it were always the best single representative of a series; whereas there are other representatives which are sometimes superior. Among these is the *middle value,* which is usually got without much calculation. It

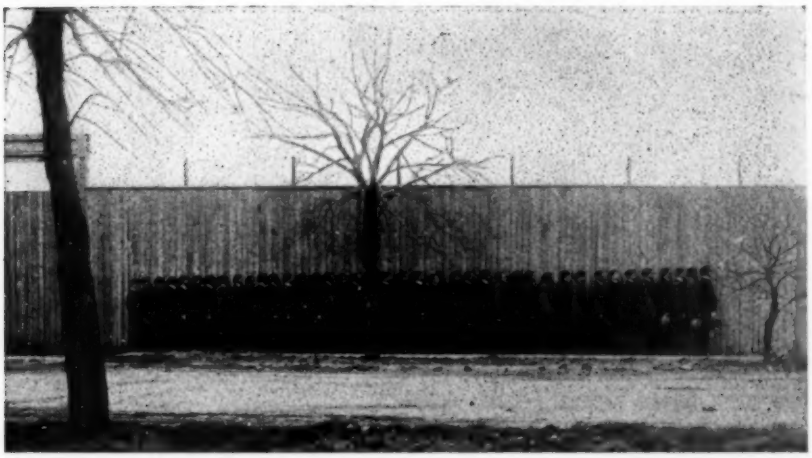

FIG. 1. FILE OF FORTY UNIVERSITY OF CHICAGO STUDENTS ARRANGED (APPROXIMATELY) IN ORDER OF HEIGHT.

is the value above and below which fifty per cent. of the cases lie. In the case of income of American colleges the middle value is not far from $15,000, while the average is $43,000; the former amount unfortunately gives the truer idea of the usual American college; for about 80 per cent. of the colleges have an income of less than $43,000 Still another representative value is the geometric mean which is especially important in many biologic and economic statistics. The geometric mean is the number corresponding to the *average of the logarithms* of the individual quantities.

Finally, if there is one representative of a biological series that is more apt to be significant than any other, it is the value that occurs with the greatest frequency or, in other words, the commonest value. Since this value may be said figuratively to be the most fashionable one,

it has been called the *mode*. The peculiar value of the mode lies in this, that it is not the result of calculation and is not an ideal value merely, but is the prevailing or typical actual condition. In biological statistics the mode should always be considered.

No single value can, however, adequately take the place of all the values obtained. Nevertheless, it is necessary to combine these data in some unit for purposes of comparison. The best unit is the so-called 'frequency polygon.' The frequency polygon is got first by sorting out the data into a number of equally extensive 'classes'; then by laying off these classes as a series of points at equal intervals of space along a horizontal base-line; by erecting perpendiculars proportional to the frequency of each class, and by joining with a line the tops of all these perpendiculars. Or, if the tops be united by a flowing line, the frequency polygon is replaced by the frequency curve.

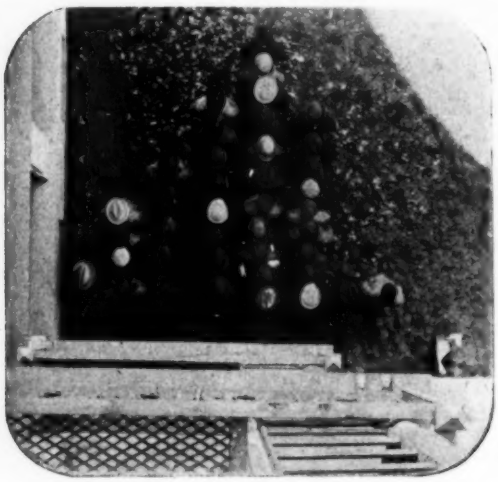

FIG. 2. BIRD'S-EYE VIEW OF 40 UNIVERSITY OF CHICAGO STUDENTS ARRANGED IN FILES BY CLASSES OF STATURE.

Such frequency polygons may also be obtained, without drawing on paper, by putting the individuals belonging to the same class in the same vertical column and arranging the columns in order along a common base-line. For example, we may separate our university students into stature classes as follows: 56 to 57.9 inches, 58 to 59.9, 60 to 61.9; 62 to 63.9; 64 to 65.9; 66 to 67.9; 68 to 69.9; 70 to 71.9; place those falling into the same stature class in a file; and place the files next each other in order, all starting from a common base-line. Then if we take a bird's-eye view of this body of students, we get the frequency polygon of their statures (Fig. 2). The construction of frequency polygons may be illustrated by another example. The common scallop shells of the Atlantic coast have a variable number of

'ribs' (Fig. 3). In any hundred individuals from one locality the number of ribs may vary from 15 to 21. If we put in one pile the shells having the same number of ribs and arrange the piles in order, from 15 to 20, upon a level base we shall get a figure which is the frequency polygon for the ribs of Pecten shells from the given locality (Fig. 4).

Frequency polygons may be obtained in the same way from measurements or countings made on almost any organ of any plant or animal. The shapes of the polygons are probably never exactly alike in different organs; consequently, there is a field for the comparing of polygons and for drawing interpretations from differences in their form.

In comparing frequency polygons attention should be directed, first of all, to two characters; namely, the position and relative proportion of individuals included in the modal class and the spread of the polygon at the base. This spread is known technically as the *range*. While

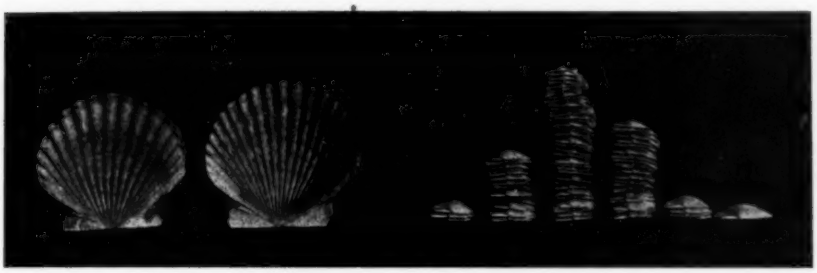

FIG. 3. SCALLOP SHELL WITH 15 AND 20 RIBS RESPECTIVELY.

FIG. 4. SELF-FORMED FREQUENCY POLYGON OF PECTEN RIBS.

some frequency polygons have a high mode and narrow range others have a low mode and a broad range. The importance of this fact is that a narrow range implies relatively small variability, since relatively few individuals depart far from the modal condition. On the other hand, wide range implies great variability. Range, however, is not an accurate measure of variability because it is too easily affected by the accidental occurrence of even one aberrant individual. We need a measure of variability that shall take into account the departures of *all* the individuals from the mode. One such measure is the arithmetical average of all the departures from the mean in both directions; and this measure has been widely employed. At present another method is preferred; namely, the square root of the average of the *squared* departures. This measure is called the standard deviation. The standard deviation is of great importance, because it is the index of variability. This index in the case of measured organs is, like the range, a concrete number; consequently indices are not always comparable, being expressed, *e. g.*, in feet, millimeters, degrees or pounds. So it has been proposed to reduce all indices (except those based on countings) to

percentages of the average value. This proportional index is called the coefficient of variability. So much then for the principal operations: counting or measuring; seriation of the numbers; determination of the mode, average, standard deviation, and coefficient of variability.

The results of comparative variation studies, so far, fall into two

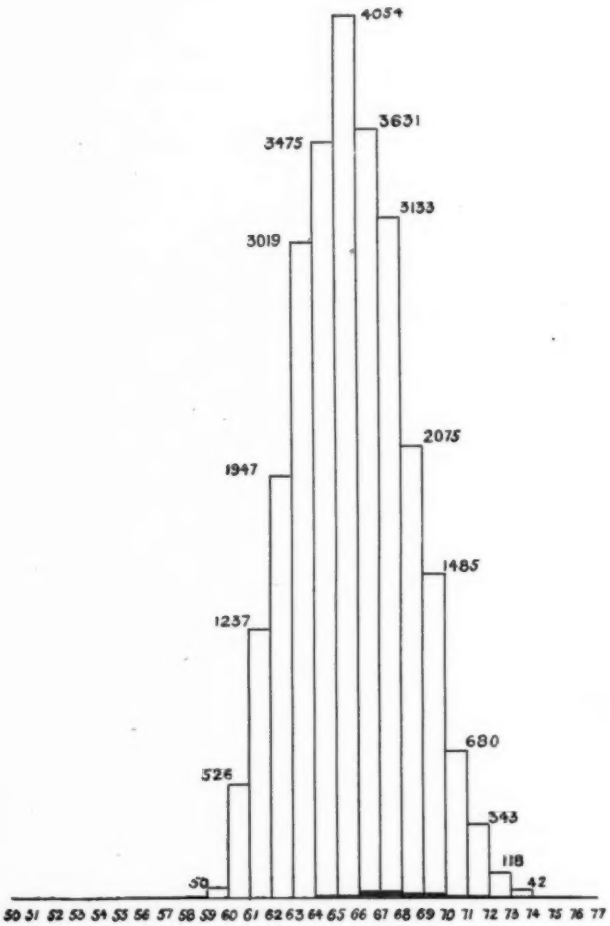

FIG. 5. FREQUENCY POLYGON OF STATURES OF 25,820 AMERICAN MALES; LOWER POLYGON, OF 100 UNIVERSITY STUDENTS.

general categories. First, we have got some notion of the classes of variation polygons that may occur among organisms; secondly, we have some evidence as to the significance of these different forms. As the science is only about five years old it is but to be expected that a satisfactory solution of even the principal problems concerning polygon

interpretation is still to be worked out. The beginnings are, however, instructive.

Variation polygons are of two main sorts—simple and complex. Simple variation polygons possess only a single mode, whilst complex

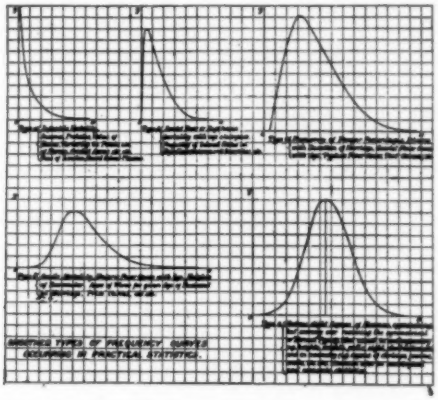

FIG. 6. TYPES OF FREQUENCY CURVES. FROM K. PEARSON.

polygons usually show a trace of more than one mode. Simple variation polygons are of several types. Some are symmetrical about the mode as in Fig. 5. Others are more or less unsymmetrical or skew, as

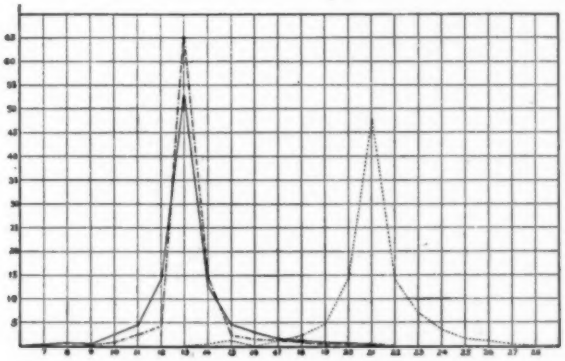

FIG. 7. CURVES OF UNSELECTED AND SELECTED LOTS OF WHITE DAISIES.
——— CURVES OF FREQUENCIES OF VARIOUS MEMBERS OF RAY FLOWERS IN WILD DAISIES.
— - — " " " " " " " " IN DESCENDANTS OF 12 OR 13-RAYED WILD DAISIES.
- - - - " " " " " " OF RAY FLOWERS IN DESCENDANTS OF 21-RAYED WILD DAISIES.

they are technically called (Fig. 6). Skew polygons usually tail out at the base further from the mode on one side than on the other. The polygon is said to be skew in the direction of this longer partial base. Why some distributions are symmetrical and others skew is not fully

understood, but at present it seems probable that we get symmetrical polygons when the organ measured is not undergoing evolution and, on the other hand, that unsymmetrical polygons indicate evolutionary progress. Also the direction of skewness is probably determined by the direction of evolution. At present, however, we can only say that in many cases the skew polygon tails off (or is skew) in the direction from which the race is evolving. This conclusion, which I believe to be new, is based upon certain results of experiments as well as upon data gathered from material which had developed under natural conditions. Of this material the most important for our purpose is that in which two polygons have apparently arisen by a splitting off from the original polygon of the two extremes which now form two distinct and widely separated types. The first case (Fig. 7) is derived from the common

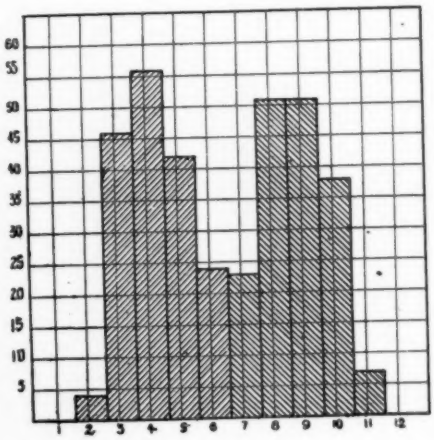

FIG. 8. POLYGON OF FREQUENCIES OF LENGTHS IN MILLIMETERS OF CEPHALIC BONES OF 343 RHINOCEROS BEETLES. FROM DATA OF BATESON.

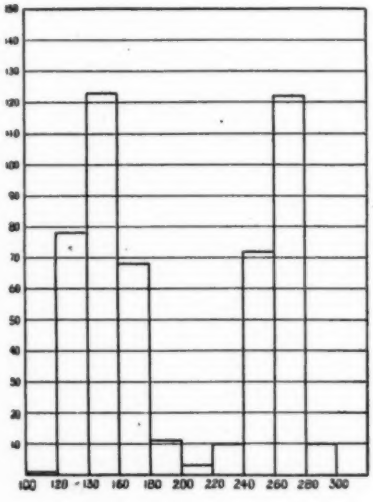

FIG. 9. POLYGON OF FREQUENCY OF LENGTHS OF WING, IN HUNDREDTHS OF A MILLIMETER, FOR A LOT OF CHINCH BUGS.

white daisy. In the figure the full-line polygon gives the frequency distribution of the ray-flowers in a collection of wild daisies. This polygon has a + skewness of 1.18. The left-hand, dot-and-dash, polygon gives the ray frequences in the descendants of 12- or 13-rayed wild plants. The positive skewness is increased as a result of this selection to + 1.92. The right-hand polygon gives the ray frequencies in the descendants of the 21-rayed plants, a single highly aberrant case of 32 rays being omitted. The skewness is — 0.13. In this case we have experimental evidence that curves are skew *towards* the original, ancestral, condition. The cases in which the frequency curve is bimodal frequently signify that two races are arising out of a former

ancestral intermediate condition; *i. e.*, they correspond to the right- and left-hand polygons of Fig. 7. Consequently we may expect them to be skew in opposite directions and so we find them to be. For example, Bateson has measured the horns on the heads of 343 rhinoceros beetles; the frequency curve is shown in Fig. 8. The left-hand polygon has a skewness of + 0.48; the right-hand polygon of — 0.03. One might infer that the right-hand form, the long-horned beetles, had diverged less from the ancestral condition than the short-horned beetles. Again, my pupil, Mr. Garber, has obtained a bimodal distribution polygon in the length of the chinch bug's wing (Fig. 9). The

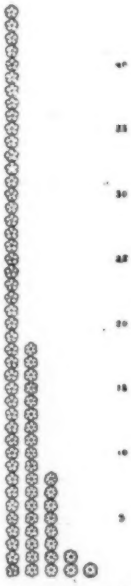

FIG. 10. FREQUENCY POLYGON OF THE NUMBER OF PETALS IN BUTTERCUPS.

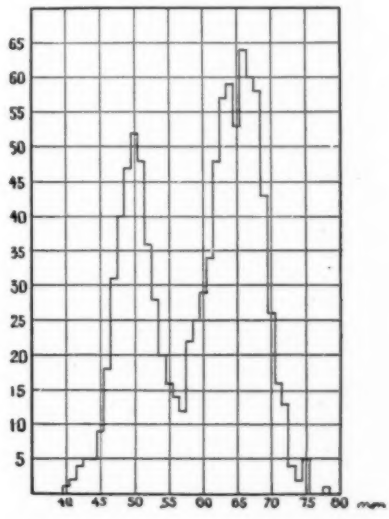

FIG. 11. POLYGON OF FREQUENCY OF LENGTHS IN MILLIMETERS, OF PECTEN SHELLS GATHERED AT RANDOM FROM A FISHERMAN'S SHELL HEAP.

short-winged form has a skewness of + .44; the long winged form of — .43. In this case also the ancestral form lies between the present modes. It is obvious that we may get cases in which two modes, representing conditions in different places, have moved, to different extents, in the same direction. Thus the index (breadth ÷ length) of the shell of Littorina, a marine snail, as measured by Bumpus, has at Newport a mode of 90; at Casco Bay of 93. The skewness is positive in both places and greater (+ .24) at the more southern point than at Casco Bay (+ .13). This result indicates that the Littorina came from a more northern home, for which we have confirmatory historical evidence, and that these ancestral races were rounder, having an index

possibly not far from 96. Likewise the Littorinas from South Kincardineshire, Scotland, have a modal index of 88 and a positive skewness of 0.065; while those of the Humber, having a mode of 91, have a skewness of + 0.048. These figures suggest an ancestral index of about 97, or about the same as before. The form of the frequency polygon may thus enable us to infer the ancestral condition of a race or species and may consequently help us to get at the history of the race.

Skewness may, as we have seen, depart to any extent from a nearly symmetrical condition. The extreme case occurs when the mode lies at one end of the range. This case is sometimes found among plants. It indicates that the group has in respect to the character in question reached an extreme condition (Fig. 10).

Complex frequency polygons have various interpretations. We have already seen that one of these interpretations at least is a splitting of one race into two. Another kind of complex polygon is due to

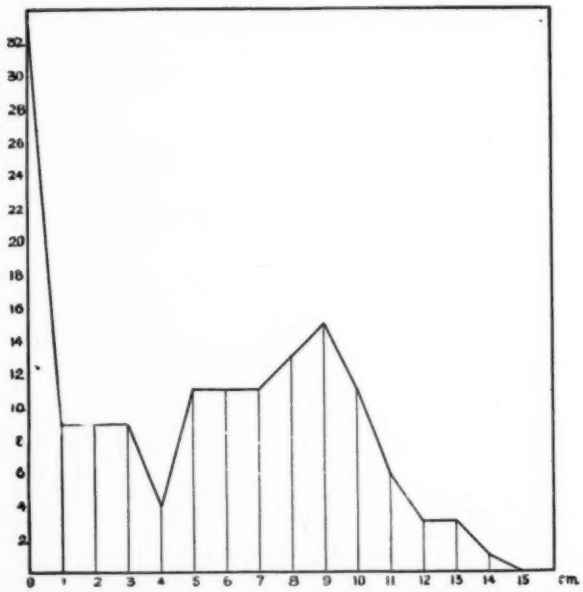

FIG. 12. COMPLEX CURVE OF LENGTH IN CM. OF FASCIATED STEM OF 146 INDIVIDUALS OF CREPIS BIENNIS DERIVED FROM FASCIATED ANCESTORS. ONE INDIVIDUAL OF 19 CENTIMETERS OMITTED. FROM DE VRIES, '95, BULLETIN SCIENTIFIQUE, TOME 27, P. 397. ORDINATES, NUMBER OF INDIVIDUALS; ABSCISSÆ, LENGTH OF FASCIATED STEM.

differences of age. Suppose an animal that breeds at one restricted season of the year and that annually nearly doubles in size. If we make a collection of a lot of these animals from a place at one time, we shall include individuals of different ages such as, for instance, six months, one year and six months, two years and six months, and so

on. If now the length of all these individuals be measured we shall obtain a series of modes of which each corresponds to one of the broods (Fig. 11). Still again, two modes may appear when the material is not perfectly homogeneous although the age be constant. For instance the material may contain both normal and abnormal individuals. An example of this sort of polygon is given in Fig. 12. A very complex curve is afforded by the number of the ray flowers of composite plants. If the lappets of a thousand white daisies be counted it will be found that there is not a single mode only but a series of them. These modes increase in height from one extremity of the range, reach a great mode at one point and then diminish again (Fig. 13). It appears also that the modes do not occur at haphazard,

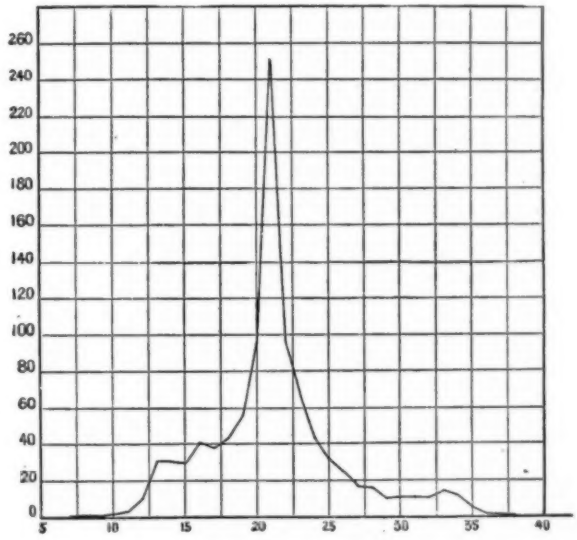

FIG. 13. POLYGON OF FREQUENCY OF NUMBERS OF RAY FLOWERS OF THE WHITE DAISY GATHERED AT RANDOM FROM VARIOUS GERMAN LOCALITIES. FROM DATA OF LUDWIG.

but chiefly in the series of numbers: 1, 2, 3, 5, 8, 13, 21, 44, and 65. This is a mathematical series in which each term is the sum of the two preceding. Also the ratios of these numbers, namely, ½, ⅓, ⅔, ⅜, and so on, have long been known to represent the arrangement of leaves on a stem; and this seems to be why the numbers of this series are so prominent in the rays of the flower head.

The comparative study of frequency polygons, such as we have been making, enables us, it will be seen, to distinguish different kinds of variation and to make that philosophical classification which is the first step in advancing knowledge. Although the causes of variation are not at once revealed, we are directed to working hypotheses that

can be tested by experiment. It is not too much to say that the frequency polygon is the key to the first door that has barred true progress in the difficult subject of the origin of organic diversity.

In what has gone before we have considered variation of single organs or qualities of a species. Yet, although we have to study the variation of organs taken one at a time, in nature no organ undergoes variation by itself alone. For the parts of the body are so knit together, their morphological kinship or their physiological interdependence is such, that when one organ deviates from the mode many others deviate also. This fact has long been known as *correlation* of variation. A recognition of the law by Cuvier was the justification, slight though it was, of his premature attempts to reconstruct an extinct form from one of its bones. Now, correlation is of great importance in the origin of species; it makes it easier to understand how evolution can take place. For example, when it was objected that natural selection by acting on one part at a time could hardly build up so complex a structure as the eye with so many mutually dependent parts, Darwin was able to rejoin that the principle of correlation comes in to ensure that when any one part is improved all other parts shall vary to meet the new conditions. And in general, a knowledge of correlation is necessary in order to complement the study of individual variation and to perfect our investigations upon the origin of species. And correlation must be studied quantitatively. A proper method has been afforded by Galton and Pearson. That method may be briefly stated. Let us suppose that we desire to find the degree of correlation in variation, or deviation from the mean, between an organ *A*, called subject, and a second organ *B*, called relative. We first take all the individuals of one (subject) class; that is, individuals whose subject organ deviates from the mean by a constant quantity, *p*. We next find for those individuals the average deviation-from-the-mean of the organ *B*, and call it *q*. We then find the ration *q/p;* this is the partial index of correlation. We find this ratio for every subject class. The average of the ratios is the index of correlation sought. The ratio will not exceed unity; because *q* is bound in the long run not to exceed *p*. When $q = p$, correlation is perfect and is equal to 1. When the index of correlation is zero, correlation is absent; when the index is negative, correlation is inverse and a large organ is associated with a small one. A good example of organs with strong positive correlation is the right and left arm. Inverse correlation is rarer; an example is stature and cephalic index. The results of studying correlation quantitatively are interesting, as showing how intimately bound together the most remote parts of the body are. Take for example the following table of correlation of parts of the human skeleton, from Pearson's 'Grammar of Science.'

Femur and tibia	.81 to .89
Femur and humerus	.84 to .87
Humerus and radius	.74 to .84
Humerus and ulna	.75 to .86
Clavicle and humerus	.44 to .63
Clavicle and scapula	.12 to .16
Stature and femur	.80 to .81
Stature and humerus	.77 to .81
Stature and fore-arm	.37
Stature and cephalic index	—.80
Length and breadth of skull	.29 to .49
Breadth and height of skull	.10 to .34
Length and capacity of skull	.50 to .89
Length x breadth x height and capacity of skull	.70 to .80
Weight and length (babies)	.62 to .64
Weight and stature (adolescents)	.50 to .72
Right and left femur	.96
Right and left first joint of ring finger	.93
First joints of right hand, index and middle fingers	.90
First joints of right hand, index and little fingers	.82
Metacarpal phalanges, right hand, index and middle fingers	.94
Metacarpal phalanges, right hand index and little fingers	.89
Strength of pull and stature	.22 to .30
Strength of pull and weight	.34 to .54

A study of this table shows us how justified was Darwin's contention that the evolution of one organ necessarily means the evolution of many parts of the body.

The modern methods of studying evolution have still another application. It is sometimes said that variation and heredity are the two factors of evolution. Heredity is, however, only a special case of correlated variation; a correlation between parents and offspring or between any two blood relatives. So evolution is reduced to a single factor—variation, simple and correlated.

As a criticism of the new methods of studying variation it has been urged that, after all, they deal not so much with the causes of evolution as with the mere results. To this criticism it may be rejoined that the first step toward the determination of the causes of a phenomenon is a precise knowledge of the limitations and conditions of the phenomenon itself; and this is what the quantitative study of variation gives. Science has been more retarded by wasted efforts to explain erroneous data than by conscientious attempts to discover the precise facts. For when the facts are correctly known the true explanation often follows at once. Even if the explanation does not follow at once the proper direction of experimentation to discover causes is indicated. Statistics tell us not only the exact static condition of species to-day under the varying circumstances of environment; but they will enable us to measure precisely the results of any change in environment, artificially or naturally brought about. We

shall thus be able not only to tell what are the factors of phylogenetic change, but also the rate of such change. We shall get possession of the laws of evolution so that we can not only reconstruct the past, but also predict the future development of a race.

The importance of knowing the methods of evolution is partly theoretical, like the importance of astronomical investigation, and partly practical. For, on the one hand, a rapid and thoroughgoing improvement of the human race can probably be effected only by understand and applying these methods; and on the other hand, the improvement of live-stock and of food plants must depend on a knowledge of the laws of phylogenesis. How appalling is our ignorance, for example, concerning the effect of a mixing of races as contrasted with pure breeding; a matter of infinite importance in a country like ours containing numerous races and subspecies of men. How little do we know of the direct effect of climate on 'blood'; a matter of concern in a land with such diversified geography. In our fast-filling earth all problems will some day be secondary to that of raising more grain or beef to the acre; then at least the biologic-evolutionary problems will be recognized as paramount. It is for us to anticipate in part the future demands on biology. The State Experiment Stations of our day are doing something in this direction, but for the most part in too narrow a fashion. For the future, broad, far-reaching experiments in evolution are required, with a quantitative study of causes and results.

THE COMBATING OF TUBERCULOSIS.

IN THE LIGHT OF THE EXPERIENCE THAT HAS BEEN GAINED IN THE SUCCESSFUL COMBATING OF OTHER INFECTIOUS DISEASES.*

By Professor ROBERT KOCH,

DIRECTOR OF THE INSTITUTION FOR INFECTIOUS DISEASES, BERLIN.

THE task with which this Congress will have to busy itself is one of the most difficult, but it is also one in which labor is most sure of its reward. I need not point again to the innumerable victims tuberculosis annually claims in all countries, nor to the boundless misery it brings on the families it attacks. You all know that there is no disease which inflicts such deep wounds on mankind as this. All the greater, however, would be the general joy and satisfaction if the efforts that are being made to rid mankind of this enemy, which consumes its inmost marrow, were crowned with success. There are many, indeed, who doubt the possibility of successfully combating this disease, which has existed for thousands of years and has spread all over the world. This is by no means my opinion. This is a conflict into which we may enter with a surely-founded prospect of success, and I will tell you the reasons on which I base this conviction. Only a few decades ago the real nature of tuberculosis was unknown to us; it was regarded as a consequence, as the expression, so to speak, of social misery, and as this supposed cause could not be got rid of by simple means people relied on the probable gradual improvement of social conditions and did nothing. All this is altered now. We know that social misery does indeed go far to foster tuberculosis, but the real cause of the disease is a parasite—that is, a visible and palpable enemy which we can pursue and annihilate, just as we can pursue and annihilate other parasitic enemies of mankind.

Strictly speaking, the fact that tuberculosis is a preventable disease ought to have become clear as soon as the tubercle bacillus was discovered and the properties of this parasite and the manner of its transmission became known. I may add that I, for my part, was aware of the full significance of this discovery from the first, and so will everybody have been who had convinced himself of the causal relation between tuberculosis and the tubercle bacillus. But the strength of a

* An address delivered before the British Congress on Tuberculosis on July 23.

small number of medical men was inadequate to the conflict with a disease so deeply rooted in our habits and customs. Such a conflict requires the cooperation of many, if possible of all, medical men, shoulder to shoulder with the State and the whole population, and now the moment when such cooperation is possible seems to have come. I suppose there is hardly any medical man now who denies the parasitic nature of tuberculosis, and among the non-medical public, too, the knowledge of the nature of the disease has been widely propagated. Another favorable circumstance is that success has recently been achieved in the combating of several parasitic diseases and that we have learned from these examples how the conflict with pestilences is to be carried on. The most important lesson we have learned from the said experience is that it is a great blunder to treat pestilences according to a general scheme. This was done in former times. No matter whether the pestilence in question was cholera, plague, or leprosy, isolation, quarantine, useless disinfection were always resorted to. But now we know that every disease must be treated according to its own special individuality and that the measures to be taken against it must be most accurately adapted to its special nature, to its etiology. We are entitled to hope for success in combating tuberculosis only if we keep this lesson constantly in view. As so extremely much depends just on this point I shall take the liberty to illustrate it by several examples.

The pestilence which is at this moment in the foreground of interest, the bubonic plague, may be instructive to us in several respects. People used to act upon the conviction that a plague patient was in the highest degree a center of infection, and that the disease was transmitted only by plague patients and their belongings. Even the most recent international agreements are based on this conviction. Although, as compared with formerly, we now have the great advantage that we can, with the aid of the microscope and of experiments on animals, recognize every case of plague with absolute certainty, and although the prescribed inspection of ships, quarantine, the isolation of patients, the disinfection of infected dwellings and ships, are carried out with the utmost care, the plague has, nevertheless, been transmitted everywhere, and has in not a few places assumed grave dimensions. Why this has happened we know very well, owing to the experience quite recently gained as to the manner in which the plague is transmitted. It has been discovered that only those plague patients who suffer from plague-pneumonia—a condition which is fortunately infrequent—are centers of infection, and that the real transmitters of the plague are the rats. There is no longer any doubt that in by far the majority of the cases in which the plague has been transmitted by ocean traffic the transmission took place by means of plague among the ship rats. It has also been found that wherever the rats were intentionally or unin-

tentionally exterminated the plague rapidly disappeared; whereas at other places where too little attention had been paid to the rat plague the pestilence continued. This connection between the human plague and the rat plague was totally unknown before, so that no blame attaches to those who devised the measures now in force against the plague if the said measures have proved unavailing. It is high time, however, that this enlarged knowledge of the etiology of the plague should be utilized in international as well as in other traffic. As the human plague is so dependent on the rat plague it is intelligible that protective inoculation and the application of antitoxic serum have had so little effect. A certain number of human beings may have been saved from the disease by that, but the general spread of the pestilence has not been hindered in the least.

With cholera the case is essentially different; it may under certain circumstances be transmitted directly from human beings to other human beings, but its main and most dangerous propagator is water, and therefore in the combating of cholera water is the first thing to be considered. In Germany, where this principle has been acted on, we have succeeded for four years in regularly exterminating the pestilence (which was introduced again and again from the infected neighboring countries) without any obstruction of traffic.

Hydrophobia, too, is not void of instruction for us. Against this disease the so-called protective inoculation proper has proved eminently effective as a means of preventing the outbreak of the disease in persons already infected, but of course such a measure can do nothing to prevent infection itself. The only real way of combating this pestilence is by compulsory muzzling. In this matter also we have had the most satisfactory experience in Germany, but have at the same time seen that the total extermination of the pestilence can be achieved only by international measures, because hydrophobia, which can be very easily and rapidly suppressed, is always introduced again year after year from the neighboring countries.

Permit me to mention only one other disease, because it is etiologically very closely akin to tuberculosis, and we can learn not a little for the furtherance of our aims from its successful combating. I mean leprosy. It is caused by a parasite which greatly resembles the tubercle bacillus. Just like tuberculosis, it does not break out till long after infection and its course is almost slower. It is transmitted only from person to person, but only when they come into close contact, as in small dwellings and bedrooms. In this disease, accordingly, immediate transmission plays the main part; transmission by animals, water, or the like is out of the question. The combative measures, accordingly, must be directed against this close intercourse between the sick and the healthy. The only way to prevent this intercourse is to isolate the

patients. This was most rigorously done in the Middle Ages by means of numerous leper-houses, and the consequence was that leprosy, which had spread to an alarming extent, was completely stamped out in Central Europe. The same method has been adopted quite recently in Norway, where the segregation of lepers has been ordered by a special law. But it is extremely interesting to see how this law is carried out. It has been found that it is not at all necessary to execute it strictly, for the segregation of only the worst cases, and even of only a part of these, sufficed to produce a diminution of leprosy. Only so many infectious cases had to be sent to the leper-houses that the number of fresh cases kept regularly diminishing from year to year. Consequently the stamping-out of the disease has lasted much longer than it would have lasted if every leper had been inexorably consigned to a leper-house, as in the Middle Ages, but in this way, too, the same purpose is gained, slowly indeed, but without any harshness.

These examples may suffice to show what I am driving at, which is to point out that in combating pestilences we must strike the root of the evil and must not squander force in subordinate ineffective measures. Now the question is whether what has hitherto been done and what is about to be done against tuberculosis really strikes the root of tuberculosis so that it must sooner or later die. In order to answer this question it is necessary first and foremost to inquire how infection takes place in tuberculosis. Of course, I presuppose that we understand by tuberculosis only those morbid conditions which are caused by the tubercle bacillus. In by far the majority of cases of tuberculosis the disease has its seat in the lungs, and has also begun there. From this fact it is justly concluded that the germs of the disease—*i. e.*, the tubercle bacilli—must have got into the lungs by inhalation. As to the question where the inhaled tubercle bacilli have come from, there is also no doubt. On the contrary, we know with certainty that they get into the air with the sputum of consumptive patients. This sputum, especially in advanced stages of the disease, almost always contains tubercle bacilli, sometimes in incredible quantities. By coughing and even speaking it is flung into the air in little drops—*i. e.*, in a moist condition—and can at once infect persons who happen to be near the coughers. But then it may also be pulverized when dry, in the linen or on the floor for instance, and get into the air in the form of dust. In this manner a complete circle, a so-called *circulus vitiosus*, has been formed for the process of infection from the diseased lung, which produces phlegm and pus containing tubercle bacilli, to the formation of moist and dry particles (which in virtue of their smallness can keep floating a good while in the air), and finally to new infection if particles penetrate with the air into a healthy lung and originate the disease anew. But the tubercle bacilli may get to other organs of the

body in the same way and thus originate other forms of tuberculosis. This, however, is a considerably rarer case. The sputum of consumptive people, then, is to be regarded as the main source of the infection of tuberculosis. On this point, I suppose, all are agreed. The question now arises whether there are not other sources, too, copious enough to demand consideration in the combating of tuberculosis.

Great importance used to be attached to the hereditary transmission of tuberculosis. Now, however, it has been demonstrated by thorough investigation that, though hereditary tuberculosis is not absolutely nonexistent, it is nevertheless extremely rare, and we are at liberty in considering our practical measures to leave this form of origination entirely out of account. But another possibility of tuberculous infection exists, as is generally assumed, in the transmission of the germs of the disease from tuberculous animals to man. This manner of infection is generally regarded nowadays as proved and as so frequent that it is even looked upon by not a few as the most important, and the most rigorous measures are demanded against it. In this Congress also the discussion of the danger with which the tuberculosis of animals threatens man will play an important part. Now, as my investigations have led me to form an opinion deviating from that which is generally accepted, I beg your permission, in consideration of the great importance of this question, to discuss it a little more thoroughly.

Genuine tuberculosis has hitherto been observed in almost all domestic animals, and most frequently in poultry and cattle. The tuberculosis of poultry, however, differs so much from human tuberculosis that we may leave it out of account as a possible source of infection for man. So, strictly speaking, the only kind of tuberculosis remaining to be considered is the tuberculosis of cattle which, if really transferable to man, would indeed have frequent opportunities of infecting human beings through the drinking of the milk and the eating of the flesh of diseased animals. Even in my first circumstantial publication on the etiology of tuberculosis I expressed myself regarding the identity of human tuberculosis and bovine tuberculosis with reserve. Proved facts which would have enabled me sharply to distinguish these two forms of the disease were not then at my disposal, but sure proofs of their absolute identity were equally undiscoverable, and I therefore had to leave this question undecided. In order to decide it I have repeatedly resumed the investigations relating to it, but so long as I experimented on small animals, such as rabbits and guinea pigs, I failed to arrive at any satisfactory result, though indications which rendered the difference of the two forms of tuberculosis probable were not wanting. Not till the complaisance of the Ministry of Agriculture enabled me to experiment on cattle, the only animals really suitable for these investigations, did I arrive at absolutely conclusive results.

Of the experiments which I have carried out during the last two years along with Professor Schütz of the Veterinary College in Berlin I will tell you briefly some of the most important.

A number of young cattle which had stood the tuberculin test, and might therefore be regarded as free from tuberculosis, were infected in various ways with tubercle bacilli taken from cases of human tuberculosis; some of them got the tuberculous sputum of consumptive patients direct. In some cases the tubercle bacillus or the sputum was injected under the skin, in others into the peritoneal cavity, in others into the jugular vein. Six animals were fed with tuberculous sputum almost daily for seven or eight months; four repeatedly inhaled great quantities of bacilli, which were distributed in water and scattered with it in the form of spray. None of these cattle (there were 19 of them) showed any symptoms of disease and they gained considerably in weight. From six to eight months after the beginning of the experiments they were killed. In their internal organs not a trace of tuberculosis was found. Only at the places where the injections had been made small suppurative foci had formed, in which few tubercle bacilli could be found. This is exactly what is found when dead tubercle bacilli are injected under the skin of animals liable to contagion. So the animals we experimented on were affected by the living bacilli of human tuberculosis exactly as they would have been by dead ones; they were absolutely insusceptible to them.

The result was utterly different, however, when the same experiment was made on cattle free from tuberculosis with tubercle bacilli that came from the lungs of an animal suffering from bovine tuberculosis. After an incubation-period of about a week the severest tuberculous disorders of the internal organs broke out in all the infected animals. It was all one whether the infecting matter had been injected only under the skin or into the peritoneal cavity or the vascular system. High fever set in and the animals became weak and lean; some of them died after from one and a half to two months; others were killed in a miserably sick condition after three months. After death extensive tuberculous infiltrations were found at the place where the injections had been made and in the neighboring lymphatic glands, and also far-advanced alterations of the internal organs, especially of the lungs and the spleen. In the cases in which the injection had been made into the peritoneal cavity the tuberculous growths which are so characteristic of bovine tuberculosis were found on the omentum and peritoneum. In short, the cattle proved just as susceptible to infection by the bacillus of bovine tuberculosis as they have proved insusceptible to infection by the bacillus of human tuberculosis. I wish only to add that preparations of the organs of the cattle which were artificially infected with bovine tuberculosis in these experiments are exhibited in the museum of pathology and bacteriology.

An almost equally striking distinction between human and bovine tuberculosis was brought to light by a feeding experiment with swine. Six young swine were fed daily for three months with the tuberculous sputum of consumptive patients. Six other swine received bacilli of bovine tuberculosis with their food daily for the same period. The animals that were fed with sputum remained healthy and grew lustily, whereas those that were fed with the bacilli of bovine tuberculosis soon became sickly, were stunted in their growth, and half of them died. After three and a half months the surviving swine were all killed and examined. Among the animals that had been fed with sputum no trace of tuberculosis was found, except here and there little nodules in the lymphatic glands of the neck and in one case a few grey nodules in the lungs. The animals, on the other hand, which had eaten bacilli of bovine tuberculosis had, without exception (just as in the cattle experiment), severe tuberculous diseases, especially tuberculous infiltration of the greatly enlarged lymphatic glands of the neck and of the mesenteric glands, and also extensive tuberculosis of the lungs and the spleen.

The difference between human and bovine tuberculosis appeared not less strikingly in a similar experiment with asses, sheep and goats, into whose vascular systems the two kinds of tubercle bacilli were injected.

Our experiments, I must add, are not the only ones that have led to this result. If one studies the older literature of the subject, and collates the reports of the numerous experiments that were made in former times by Chauveau, Günther and Harms, Bollinger and others, who fed calves, swine and goats with tuberculous material, one finds that the animals that were fed with the milk and pieces of the lungs of tuberculous cattle always fell ill of tuberculosis, whereas those that were fed with human material did not. Comparative investigations regarding human and bovine tuberculosis have been made very recently in North America by Smith, Dinwiddie, Frothingham and Repp, and their result agreed with that of ours. The unambiguous and absolutely conclusive result of our experiments is due to the fact that we chose methods of infection which excluded all sources of error, and carefully avoided everything connected with the stalling, feeding and tending of the animals that might have a disturbing effect on the experiments. Considering all these facts, I feel justified in maintaining that human tuberculosis differs from bovine and cannot be transmitted to cattle. It seems to me very desirable, however, that these experiments should be repeated elsewhere, in order that all doubts as to the correctness of my assertion may be removed. I wish only to add that, owing to the great importance of this matter, our Government has resolved to appoint a commission to make further inquiries on the subject.

But, now, how is it with the susceptibility of man to bovine tuberculosis? This question is far more important to us than that of the susceptibility of cattle to human tuberculosis, highly important as that is too. It is impossible to give this question a direct answer, because, of course, the experimental investigation of it with human beings is out of the question. Indirectly, however, we can try to approach it. It is well known that the milk and butter consumed in great cities very often contain large quantities of the bacilli of bovine tuberculosis in a living condition, as the numerous infection-experiments with such dairy products on animals have proved. Most of the inhabitants of such cities daily consume such living and perfectly virulent bacilli of bovine tuberculosis, and unintentionally carry out the experiment which we are not at liberty to make. If the bacilli of bovine tuberculosis were able to infect human beings, many cases of tuberculosis caused by the consumption of alimenta containing tubercle bacilli could not but occur among the inhabitants of great cities, especially the children. And most medical men believe that this is actually the case.

In reality, however, it is not so. That a case of tuberculosis has been caused by alimenta can be assumed with certainty only when the intestine suffers first—*i. e.*, when a so-called primary tuberculosis of the intestines is found. But such cases are extremely rare. Among many cases of tuberculosis examined after death I myself remember having seen primary tuberculosis of the intestine only twice. Among the great post-mortem material of the Charité Hospital in Berlin 10 cases of primary tuberculosis of the intestine occurred in five years. Among 933 cases of tuberculosis in children at the Emperor Frederick's Hospital for Children Baginsky never found tuberculosis of the intestine without simultaneous disease of the lungs and the bronchial glands. Among 3,104 post-mortem examinations of tuberculous children Biedert observed only 16 cases of primary tuberculosis of the intestine. I could cite from the literature of the subject many more statistics of the same kind, all indubitably showing that primary tuberculosis of the intestine, especially among children, is a comparatively rare disease, and of the few cases that have been enumerated it is by no means certain that they were due to infection by bovine tuberculosis. It is just as likely that they were caused by the widely-propagated bacilli of human tuberculosis, which may have got into the digestive canal in some way or other—for instance, by swallowing saliva of the mouth. Hitherto nobody could decide with certainty in such a case whether the tuberculosis of the intestine was of human or of animal origin. Now we can diagnose the two. All that is necessary is to cultivate in pure culture the tubercle bacilli found in the tuberculous material and to ascertain whether they belong to bovine tuberculosis by inoculating cattle with them. For this purpose I recom-

mend subcutaneous injection which yields quite specially characteristic and convincing results. For half a year past I have occupied myself with such investigations, but owing to the rareness of the disease in question the number of the cases which I have been able to investigate is but small. What has hitherto resulted from this investigation does not speak for the assumption that bovine tuberculosis occurs in man.

Though the important question whether man is susceptible to bovine tuberculosis at all is not yet absolutely decided, and will not admit of absolute decision to-day or to-morrow, one is nevertheless already at liberty to say that, if such a susceptibility really exists the infection of human beings is but a very rare occurrence. I should estimate the extent of infection by the milk and flesh of tuberculous cattle and the butter made of their milk as hardly greater than that of hereditary transmission, and I therefore do not deem it advisable to take any measures against it.

So the only main source of the infection of tuberculosis is the sputum of consumptive patients and the measures for the combating of tuberculosis must aim at the prevention of the dangers arising from its diffusion. Well, what is to be done in this direction? Several ways are open. One's first thought might be to consign all persons suffering from tuberculosis of the lungs whose sputum contains tubercle bacilli to suitable establishments. This, however, is not only absolutely impracticable but also unnecessary. For a consumptive who coughs out tubercle bacilli is not necessarily a source of infection on that account so long as he takes care that his sputum is properly removed and rendered innocuous. This is certainly true of very many patients, especially in the first stages, and also of those who belong to the well-to-do classes and are able to procure the necessary nursing. But how is it with people of very small means? Every medical man who has often entered the dwellings of the poor, and I can speak on this point from my own experience, knows how sad is the lot of consumptives and their families there. The whole family have to live in one or two small, ill-ventilated rooms. The patient is left without the nursing he needs because the able-bodied members of the family must go to their work. How can the necessary cleanliness be secured under such circumstances? How is such a helpless patient to remove his sputum so that it may do no harm? But let us go a step further and picture the condition of a poor consumptive patient's dwelling at night. The whole family sleep crowded together in one small room. However cautious he may be the sufferer scatters the morbid matter secreted by his diseased lungs every time he coughs and his relatives close beside him must inhale this poison. Thus whole families are infected. They die out and awaken in the minds of those who do not know the infectious-

ness of tuberculosis the opinion that it is hereditary, whereas its transmission in the cases in question was due solely to the simplest processes of infection, which do not strike people so much because the consequences do not appear at once, but generally only after the lapse of years.

Often in such circumstances the infection is not restricted to a single family, but spreads in densely inhabited tenement houses to the neighbors, and then, as the admirable investigations of Biggs have shown in the case of the densely peopled parts of New York, regular nests or foci of disease are formed. But if one investigates these matters more thoroughly one finds that it is not poverty *per se* that favors tuberculosis, but the bad domestic conditions under which the poor everywhere, but especially in great cities, have to live. For, as the German statistics show, tuberculosis is less frequent, even among the poor, when the population is not densely packed together, and may attain very great dimensions among a well-to-do population when the domestic conditions, especially as regards the bedrooms, are bad, as is the case, for instance, among the inhabitants of the North Sea coast. So it is the overcrowded dwellings of the poor that we have to regard as the real breeding-places of tuberculosis; it is out of them that the disease always crops up anew, and it is to the abolition of these conditions that we must first and foremost direct our attention if we wish to attack the evil at its root and to wage war against it with effective weapons. This being so, it is very gratifying to see how efforts are being made in almost all countries to improve the domestic conditions of the poor. I am also convinced that these efforts, which must be promoted in every way, will lead to a considerable diminution of tuberculosis. But a long time must elapse ere essential changes can be effected in this direction, and much may be done meanwhile in order to reach the goal much more rapidly.

If we are not able at present to get rid of the danger which small and overcrowded dwellings involve, all we can do is to remove the patients from them and, in their own interests and that of the people about them, to lodge them better, and this can be done only in suitable hospitals. But the thought of attaining this end by compulsion of any kind is very far from me; what I want is that they may be enabled to obtain the nursing they need better than they can obtain it now. At present a consumptive in an advanced stage of the disease is regarded as incurable and as an unsuitable inmate for a hospital. The consequence is that he is reluctantly admitted and dismissed as soon as possible. The patient, too, when the treatment seems to him to produce no improvement and the expenses, owing to the long duration of his illness, weigh heavily upon him, is himself animated by the wish to leave the hospital soon. That would be altogether altered if

we had special hospitals for consumptives, and if the patients were taken care of there for nothing, or at least at a very moderate rate. To such hospitals they would willingly go; they could be better treated and fed there than is now the case. I know very well that the execution of the project will have great difficulties to contend with, owing to the considerable outlay it entails. But very much would be gained if, at least in the existing hospitals, which have to admit a great number of consumptives at any rate, special wards were established for them in which pecuniary facilities would be offered them. If only a considerable fraction of the whole number of consumptives were suitably lodged in this way a diminution of infection, and consequently of the sum-total of tuberculosis, could not fail to be the result. Permit me to remind you in this connection of what I said about leprosy. In the combating of that disease also great progress has already been made by lodging only a fair number of the patients in hospitals. The only country that possesses a considerable number of special hospitals for tuberculous patients is England, and there can be no doubt that the diminution of tuberculosis in England, which is much greater than in any other country, is greatly due to this circumstance. I should point to the founding of special hospitals for consumptives and the better utilization of the already existing hospitals for the lodging of consumptives as the most important measure in the combating of tuberculosis, and its execution opens a wide field of activity to the State, to municipalities, and to private benevolence. There are many people who possess great wealth and would willingly give of their superfluity for the benefit of their poor and heavily afflicted fellow-creatures, but do not know how to do this in a judicious manner. Here is an opportunity for them to render a real and lasting service by founding consumption hospitals or purchasing the right to have a certain number of consumptive patients maintained in special wards of other hospitals free of expense.

As, however, unfortunately, the aid of the State, the municipalities, and rich benefactors will probably not be forthcoming for a long time yet, we must for the present resort to other measures that may pave the way for the main measure just referred to and serve as a supplement and temporary substitute for it. Among such measures I regard obligatory notification as specially valuable. In the combating of all infectious diseases it has proved indispensable as a means of obtaining certain knowledge as to their state, especially their dissemination, their increase, and their decrease. In the conflict with tuberculosis also we cannot dispense with obligatory notification; we need it not only in order to inform ourselves as to the dissemination of this disease, but mainly in order to learn where help and instruction can be given, and especially where the disinfection which is so urgently

necessary when consumptives die or change their residences has to be effected. Fortunately it is not at all necessary to notify all cases of tuberculosis, nor even all cases of consumption, but only those that, owing to the domestic conditions, are sources of danger to the people about them. Such limited notification has already been introduced in various places—in Norway, for instance, by a special law, in Saxony by a Ministerial decree, in New York, and in several American towns which have followed its example. In New York, where notification was optional at first and was afterwards made obligatory, it has proved eminently useful. It has thus been proved that the evils which it used to be feared the introduction of notification for tuberculosis would bring about need not occur and it is devoutly to be wished that the examples I have named may very soon excite emulation everywhere.

There is another measure connected with notification—viz., disinfection, which, as already mentioned, must be effected when consumptives die or change their residence in order that those who next occupy the infected dwelling may be protected against infection. Moreover, not only the dwellings but also the infected beds and clothes of consumptives ought to be disinfected. A further measure, already recognized on all hands as effective, is the instructing of all classes of the people as to the infectiousness of tuberculosis and the best way of protecting oneself. The fact that tuberculosis has considerably diminished in almost all civilized states of late is attributable solely to the circumstance that knowledge of the contagious character of tuberculosis has been more and more widely disseminated and that caution in intercourse with consumptives has increased more and more in consequence. If better knowledge of the nature of tuberculosis has alone sufficed to prevent a large number of cases this must serve us as a significant admonition to make the greatest possible use of this means and to do more and more to bring it about that everybody may know the dangers that threaten them in intercourse with consumptives. It is only to be desired that the instructions may be made shorter and more precise than they generally are, and that special emphasis may be laid on the avoidance of the worst danger of infection, which is the use of bedrooms and small ill-ventilated workrooms simultaneously with consumptives. Of course the instructions must include directions as to what consumptives have to do when they cough and how they are to treat their sputum. Another measure, which has come into the foreground of late, and which at this moment plays to a certain extent a paramount part in all efforts for the combating of tuberculosis, works in quite another direction. I mean the founding of sanatoria for consumptives.

That tuberculosis is curable in its early stages must be regarded as an undisputed fact. The idea of curing as many tuberculous

patients as possible in order to reduce the number of those that reach the infectious stage of consumption and thus to reduce the number of fresh cases was therefore a very natural one. The only question is whether the number of persons cured in this way will be great enough to exercise an appreciable influence on the retrogression of tuberculosis. I will try to answer this question in the light of the figures at my disposal. According to the business report of the German Central Committee for the Establishment of Sanatoria for the Cure of Consumptives, about 5,500 beds will be at the disposal of these institutions by the end of 1901, and then, if we assume that the average stay of each patient will be three months, it will be possible to treat at least 20,000 patients every year. From the reports hitherto issued as to the results that have been achieved in the establishments we learn further that about 20 per cent. of the patients who have tubercle bacilli in their sputum lose them by the treatment there. This is the only sure test of success, especially as regards prophylaxis. If we make this the basis of our estimates, we find that 4,000 consumptives will leave these establishments annually as cured. But, according to the statistics ascertained by the German Imperial Office of Health, there are 226,000 persons in Germany over fifteen years of age who are so far gone in consumption that hospital treatment is necessary for them. Compared with this great number of consumptives the success of the establishments in question seems so small that a material influence on the retrogression of tuberculosis in general is not yet to be expected of them. But pray do not imagine that I wish by this calculation of mine to oppose the movement for the establishment of such sanatoria in any way. I only wish to warn against the overestimating of their importance which has recently been observable in various quarters, based apparently on the opinion that the war against tuberculosis can be waged by means of sanatoria alone and that other measures are of subordinate value. In reality the contrary is the case. What is to be achieved by the general prophylaxis resulting from recognition of the danger of infection and the consequent greater caution in intercourse with consumptives is shown by a calculation of Cornet's regarding the decrease of mortality from tuberculosis in Prussia in the years 1889 to 1897. Before 1889 the average was 31.4 per 10,000, whereas in the period named it sank to 21.8, which means that in that short space of time the number of deaths from tuberculosis was 184,000 less than was to be expected from the average of the preceding years. In New York, under the influence of the general sanitary measures directed in a simply exemplary manner by Biggs, the mortality from tuberculosis has diminished by more than 35 per cent. since 1886. And it must be remembered that both in Prussia and in New York the progress indi-

cated by these figures is due to the first beginnings of these measures. Considerably greater success is to be expected of their further development. Biggs hopes to have got so far in five years that in the city of New York alone the annual number of deaths from tuberculosis will be 3,000 less than formerly.

Now, I do indeed believe that it will be possible to render the sanatoria considerably more efficient. If strict care be taken that only patients be admitted for whom the treatment of those establishments is well adapted and if the duration of the treatment be prolonged it will certainly be possible to cure 50 per cent. and perhaps still more. But even then, and even if the number of the sanatoria be greatly increased, the total effect will always remain but moderate. The sanatoria will never render the other measures I have mentioned superfluous. If their number becomes great, however, and if they perform their functions properly, they may materially aid the strictly sanitary measures in the conflict with tuberculosis.

If now, in conclusion, we glance back once more to what has been done hitherto for the combating of tuberculosis, and forward to what has still to be done, we are at liberty to declare with a certain satisfaction that very promising beginnings have already been made. Among these I reckon the consumption hospitals of England, the legal regulations regarding notification in Norway and Saxony, the organization created by Biggs in New York (the study and imitation of which I most urgently recommend to all municipal sanitary authorities), the sanatoria, and the instruction of the people. All that is necessary is to go on developing these beginnings, to test and, if possible, to increase their influence on the diminution of tuberculosis, and wherever anything useful has yet been done to do likewise. If we allow ourselves to be continually guided in this enterprise by the spirit of genuine preventive medical science, if we utilize the experience gained in conflict with other pestilences, and aim, with clear recognition of the purpose and resolute avoidance of wrong roads, at striking the evil at its root, then the battle against tuberculosis, which has been so energetically begun, cannot fail to have a victorious issue.

THE DISCOVERY OF THE LAW OF GRAVITATION.

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THE law of gravitation, that all matter attracts all matter, directly as the mass and inversely as the square of the distance, whether we consider the extent of its reach, or the number and variety and peculiar interest of the problems of which it furnishes the solution, or the grandeur of many of those problems by reason of the magnitude of the elements involved; whether we consider the power which it gives us to anticipate nature, and predict with the minutest accuracy and certainty of a mathematical demonstration celestial phenomena for ages to come; we cannot but regard it as the most important truth in the whole book of nature and its discovery as the most interesting event in the history of physical science. As there is but one material universe and the law of gravitation solves the enigma of *its* structure, no other problem of equal interest and importance can ever occupy the attention of the student of nature.

Kepler has remarked that: "The occasions by which men have acquired a knowledge of celestial phenomena, are not less admirable than the discoveries themselves." If this be so, the history of the discovery of that great law of nature by which all celestial phenomena are determined can never cease to be a matter of peculiar interest.

In the account which we propose to give of the discovery we shall select as our chronological starting point the beginning of the seventeenth century. At that period the theory in regard to the structure of the material universe which, with few exceptions had been held from time immemorial, still prevailed. The earth was regarded as the center of the universe, about which the sun, moon, planets and stars performed their ceaseless revolutions. More than half a century before (in 1543) Copernicus, in his memorable work, 'De Orbium Cœlestium Revolutionibus,' had, indeed, announced the true system of the universe, yet as he was led to the adoption of the theory he proposed, not so much by positive evidence in its favor as by the difficulty of reconciling certain phenomena with the Ptolemaic theory; moreover, as the objections to this theory were from their very nature such that few could appreciate their force, whilst in the *apparent* motions of the heavenly bodies every one could see what seemed to be an ocular demonstration of its truth, it is not strange that the doctrine of Copernicus should have been for so long a time generally regarded as an

interesting yet fanciful speculation. It remained for a subsequent age to furnish proof of the truth of the Copernican system which could not be gainsaid or resisted.

At the beginning of the seventeenth century, the dicta of Aristotle, in regard to matters of science as well as philosophy, were still accepted, as they had been for many centuries preceding, as of infallible authority. In regard to the subject of our inquiry, he taught that bodies at the surface of the earth fell or tended to fall toward the center of the earth, not in virtue of any attraction of the earth, but in virtue of the fact that the center of the earth was the center of the material universe —that if the earth itself should be moved out of its place and then left free to move, it would return to its place by the same law of nature which controlled all terrestrial bodies. He taught moreover that celestial bodies were different in kind from bodies terrestrial—that whilst the latter were imperfect, corruptible and changeable, the former were perfect, (and therefore, according to his fancy, perfectly spherical in form) incorruptible, unchangeable and self-luminous. Being different in kind, he held that they were subject to entirely different physical laws; that whereas the motion of terrestrial bodies when free to move was rectilinear, by a necessity of their nature, the motion of celestial bodies was circular by a like necessity of their nature. His language on this point is worth quoting as an illustration of the contrast between the ancient and modern method of philosophizing in regard to natural phenomena. He says: "All simple motion must be rectilinear or circular; either to a center or from a center, each of which is rectilinear, or about a center. It is natural for two of the elements—earth and water—which are heavy, to tend to a center; two—air and fire—which are light, to tend from a center. As the motion of all terrestrial elements is therefore rectilinear, it seems reasonable that celestial bodies, which are of a different nature, should have only the other simple motion possible, namely, circular motion."

The year 1609 marks a new era in the history of astronomy. In this year two events occurred, independent, yet alike memorable as contributing to the overthrow of the theory in regard to the structure of the material universe which had previously prevailed and establishing the doctrine of Copernicus upon an immovable foundation. The invention of the telescope by Galileo, and the immediate discovery by means of it of the inequalities of the moon's surface, the phases of Venus, the satellites of Jupiter and the rings of Saturn, at once annihilated the fancies of Aristotle as to the perfectly spherical form of the planets, their self-luminosity, and their difference in kind from bodies terrestrial. The other memorable event referred to was the publication of Kepler's great work on 'The Motions of Mars,' in which, with much that was fanciful, two of the three laws of planetary motion were for

the first time announced. Some twelve years later, in his work, entitled 'Harmonies,' he announced the third law of planetary motion, fully establishing his right to the title, by which he has since been distinguished 'The Legislator of the Heavens.'

These laws of Kepler are: First, that the orbits of the planets are elliptical, the sun being at one of the foci; second, that the radius vector, that is, a line drawn from a planet to the sun, passes over equal spaces in equal times; third, that the squares of the times of revolution of the different planets, are to each other as the cubes of the mean distances from the sun.

Together with these laws of planetary motion, two of the three axioms of the science of mechanics, known as *the Laws of Motion,* were about this time discovered, or rather, were now for the first time distinctly apprehended and enunciated. The first of these was given by Kepler—the law of *inertia,* namely, that a body will persevere in the state in which it is, whether of rest or motion, until it is acted on by some force; or more precisely, a body at rest will continue at rest until acted on by some force, and when acted on by any single force, if free to move, its motion will be rectilinear, uniform and continuous until the body is acted on by some other force. The second law of motion was announced by Galileo, and is known as the law of *the coexistence of motions,* or independence of forces. It may be expressed as follows: If a body be acted on by several forces simultaneously, it will obey the impulse of each force, just as it would if the others were not acting. [The simplest illustration of this law is what is known as the parallelogram of forces. If the direction and intensity of two forces acting simultaneously on a body be represented by the sides of a parallelogram, the body will describe the diagonal of the parallelogram; that is, at the end of a unit of time the body will be just where it would have been if the forces had, each for a unit of time, acted consecutively.]

The true system of the universe, the laws of planetary motion and the fundamental principles of mechanics having become known, for the first time in the history of the race any intelligent inquiry as to the physical causes of the motions of the heavenly bodies became possible. With earnestness and assiduity proportioned to the interest and grandeur of the problem, men of science at once applied themselves to its solution, and yet half a century of gradual progress elapsed before the desired result was reached. From the facts which we shall have occasion to mention it will appear how much, or rather how little, foundation there is for the common belief that the idea of the law of gravitation was wholly original with Newton—suggested to him for the first time by observing the fall of an apple, and then suddenly coming forth from his brain like Minerva from the head of Jove, un-

heralded and complete. The ordinary method of transition from widespread and plausible error to the truth is by slow and gradual progress, and the discovery of the law of gravitation, so far from being an exception to this rule, is but one of its most striking illustrations. Such an *accident* as that which the discovery of the law of gravitation is supposed to have been is of the kind which only happens to men of large knowledge, profound thought, and after intense and protracted mental effort. Simple as this law is known to be, and easily apprehended and even demonstrated by ordinary minds, it needed one endowed with the most gigantic intellect probably ever given to mortal—availing himself of the suggestions and the results of the labors of those who had preceded him in the same field of inquiry—to make the discovery.

In tracing the history of this discovery, from the epoch when by the previous discovery of all the necessary data it for the first time became possible, the first place in the order of time, and next to Newton in the order of merit, is undoubtedly due to Kepler. Possessing a singularly lively imagination—we might say, volatile fancy—combined with a love for the truth that amounted to a ruling passion, and a breadth of knowledge in his favorite science far in advance of any other man of his age, he was eminently fitted for the work which he so successfully performed of scientific discovery. Fertile in hypotheses—sometimes the most extravagant—he was indefatigable in his labors to test his hypotheses by the facts. Without the slightest pride of opinion, he seemed to take a satisfaction in exploding his own theories when they were false, that was only exceeded by his delight when successful in demonstrating their truth. Of the men who have contributed to the advancement of science, there are few to whom we are under greater obligation, or whose character as an investigator of nature is more worthy of admiration, than 'The Legislator of the Heavens'—the father of modern astronomy.

In the introduction to his memorable work on 'The Motions of Mars,' referred to above, he opposed the doctrine of Aristotle on the subject of terrestrial gravity, and in the course of the discussion uses the following remarkable language:

> **A mathematical point, whether the center of the universe, or not, has no power to move heavy bodies to approach it. Let philosophers prove, if they can, that natural things have any sympathy with that which is nothing.**
>
> **The true theory of gravity is founded on the following axioms. *Gravity is a mutual affection between cognate bodies toward union or conjunction, similar to the magnetic virtue.* If we assume the earth to be the center of the world, heavy bodies are not carried toward its center in virtue of its quality of center of the world, but in virtue of its quality of center of a cognate round body; so that wheresoever the earth may be placed, or whithersoever it may be carried by its animal faculty (alluding to a fanciful theory which we shall have occasion presently to notice) heavy bodies will always be carried toward it.**

If the earth were not round, heavy bodies would not tend from every side toward its center, but to different points from different sides.

If two stones were placed in any part of the universe, near each other, and beyond the sphere of the influence of a third cognate body, these stones would come together at an intermediate point, each approaching the other at a distance proportional to the comparative mass of the other.

If the moon and the earth were not retained in their orbits by their annual force, or some other equivalent, the earth would mount to the moon by a fifty-fourth part of their distance from each other, and the moon would fall toward the earth through the other fifty-three parts, that is, assuming that the substance of the earth is of the same density.

The sphere of the attractive virtue which is in the moon, extends to the earth and entices up the waters, but as the moon flies rapidly across the zenith and the waters cannot follow so quickly, a flow of the ocean is occasioned toward the westward.

If the attractive virtue of the moon extends to the earth, it follows, with greater reason, that the attractive virtue of the earth extends to the moon and much farther, and in short, nothing which consists of earthy substance, however constituted, although thrown up to any height, can ever escape the powerful operation of this attractive virtue.

These views of Kepler—so novel at the time they were announced by him and yet which we now know to be in the main so correct—were published more than thirty years before Newton was born. As we read them, our first feeling is one of surprise that any subsequent investigator of the phenomena of gravitation should be able, by his discoveries, to achieve for himself a fame which should not only render his name immortal but should almost wholly hide from view the merit of the great pioneer in this field of inquiry. To appreciate, however, the importance of the work which yet remained to be performed, we should bear in mind that whilst Kepler's views in regard to terrestrial gravity were so remarkably just, he at the same time, in common with the age in which he lived, and, we may say, with all preceding ages—regarded the tendency of bodies near the earth to fall toward its center, and the motions of heavenly bodies, as entirely different phenomena and not at all referable to the same physical cause. He indeed speculated on the possibility of referring the motions of the planets to an attractive force emanating from the sun, similar to that which caused bodies near the earth to tend toward its center, and concluded that such a hypothesis was untenable, inasmuch as the motion in one case was rectilinear, and in the other curvilinear. Again, not to overestimate the merit of Kepler in connection with the discovery of the law of gravitation, we should remember that a theory as to the physical cause of natural phenomena, even if it be in the main correct, will furnish no complete solution of the problems which phenomena present, unless it express accurately and precisely the *measure* as well as the *mode* of the action of the assigned cause. For example, to know merely that all matter attracts all matter, would not enable us to

explain the phenomena of gravitation; we need to know precisely how the intensity of this attraction is affected by the comparative magnitude of the masses and by the distance of the masses from each other. Now the theory of Kepler in regard to gravity was correct as to the first of these points, namely that the intensity of this attraction was directly as the mass, but he was in error in regard to the second point, as he supposed that the intensity of the attraction was inversely as the distance, instead of what was subsequently found to be the fact, the square of the distance.

Once more, to estimate at its just value the part which Kepler performed in the discovery of the laws of gravitation, we should bear in mind, that an hypothesis, even if subsequently it be found to be correct, is of no *authority* until its truth be demonstrated. It may be of great importance, by way of suggestion, in directing the labors of subsequent inquirers, but the chief merit of the discovery of the truth is due to the individual who furnishes its demonstration. When this is done, and not before, that which was previously but an hypothesis takes its place among the recognized laws of nature.

As in Kepler's day, the tendency of bodies near the earth to fall toward its center and the motions of the heavenly bodies were regarded as phenomena of entirely different laws of nature, his views as to the physical cause of planetary motion next claim our attention. He supposed that the motion of the planets in their orbits was due to an influence emanating from the sun, but assuming that if this influence were an *attractive* force, similar to terrestrial gravity, its effect would be to cause the planets to fall toward the sun in straight lines, instead of the actual motion of revolution about the sun; he supposed that the emanation was of a corporeal nature, somewhat analogous to light; that as the sun revolved on its axis, this emanation revolved with it just as the spokes of a wheel when the hub revolves, and that the planets were swept along in their orbits by the revolution of this emanation—the force which caused them to move being a *propulsion* and not an *attraction*. As the hypothesis would seem to require that the times of revolution of all the planets should be the same, whereas they are different, the nearer performing their annual revolution in a time less than the more remote—he supposed that the density of the emanation diminished as its distance from the sun increased; that consequently its virtue, or propulsive energy, diminished in like manner, just as the intensity of light diminishes with the increase of the distance from the luminous center. This would account in a general way for the fact that the times of revolution of the planets nearer the sun are shorter than the times of revolutions of those more remote, but the precise difference in the observed times of revolution was not exactly that which would be required by the hypothesis. Moreover, he had observed

that the orbits of the planets were not circular, as would seem to be required by his hypothesis, but elliptical, the sun being at one of the foci; also, the ever-varying radius vector always passed over equal spaces in equal times, hence the motion of the planet in its orbit was not uniform, as his hypothesis would require, but ever-varying; and this variation too was evidently not fortuitous or uncertain but increased or diminished in the exact ratio to the varying distance of the planet from the sun required by the law just mentioned, of equal spaces in equal times. These facts, apparently so inconsistent with his hypothesis, Kepler accounted for by supposing that each of the planets was animated by an intelligent spirit, by whose agency the motion of the planet was, in part at least, determined. We have seen an allusion to this theory in the quotation above given, on the subject of gravity. He regarded each of the heavenly bodies, and the earth as one of them, as literally a huge animal, and in one of his works describes with some minuteness the habits of that particular animal on whose body it is our lot to live.

Kepler's hypothesis of an emanation from the sun of a corporeal nature by whose revolution the planets were propelled in their orbits was received with more or less favor for a time, but was soon superseded by another memorable hypothesis no more reasonable or plausible and yet from the time of its announcement until the publication of 'The Principia' demonstrated its fallacy, it was adopted by most men of science and may be said to have been the accepted theory on the subject. We refer to the *Vortices of Descartes.* This distinguished philosopher, born 1596, rose to eminence about the time of Kepler's death, which occurred in 1630. By the force of his genius, illustrated not only by that achievement for which his name will ever be held in honored remembrance—the invention of analytical geometry—but by the abundance and ability of his labors in every department of science and philosophy, Descartes, for more than half a century, occupied a position in the learned world scarcely inferior to that which for ages preceding had been held by Aristotle.

As to the cause of planetary motion, Descartes assumed the existence, throughout the limits of our system, of a subtle transparent fluid in ceaseless revolution about the sun as its center, and that the planets floated in this fluid and were consequently carried round by the sun in its motion, just as in a whirlpool a cork or floating body is carried round by the motion of the water. To account for the difference in the times of revolution of different planets, he supposed that the velocity of the revolution of the fluid, at different distances from the sun was different. To account for the revolution of the satellites of the planets, he assumed that in the nighborhood of each planet this fluid revolved about the planet as a center. To this purely fanciful

hypothesis there are several fatal objections, as was subsequently demonstrated by D'Alembert, of which it will be sufficient to mention that the very existence of a spherical vortex is a mechanical impossibility. And yet such was the weight of the authority of its author, and the ingenuity with which it was defended by himself and his followers, that, as was mentioned above, it not only was received with general favor but for more than half a century it was accepted by most men of science without questioning and continued to be maintained by some, even after Newton had announced and demonstrated the law of gravitation. It is a notable illustration of the tenacity of error when once it becomes firmly fixed and widespread, that for some years after the publication of 'The Principia,' a Latin translation from the French of 'The Physics of Rohault'—a work entirely Cartesian—continued to be the text-book in Philosophy at the University of Cambridge—Newton himself being at the time Lucasian Professor of Mathematics. We have the authority of Playfair for the statement (which, indeed, has been called in question by Sir David Brewster, in his 'Life of Newton,' though so far as we have been able to see, without any sufficient reason) that the doctrines of 'The Principia' were introduced into the regular course of instruction at Cambridge by strategem. Dr. Samuel Clarke, a zealous advocate of the Newtonian Philosophy, prepared a new and more elegant translation of Rohault, with copious notes, in which the doctrines of 'The Principia' were explained and defended, and it was by this work, more directly than by the lectures of Newton himself, that Cartesianism was finally driven from the University.

Whilst Kepler's speculations as to the cause of the motions of heavenly bodies were soon supplanted by the hypothesis of Descartes, his more just views in regard to terrestrial gravity commended themselves to the scientific world and speedily passed into universal and abiding favor. In the memorable work of Galileo on the true system of the universe—completed the very year after Kepler's death, and published two years after; a work which, aside from its own merit, 'The Holy Inquisition,' by the persecution of its author, has made immortal—we find the doctrine of Kepler, on the subject of gravity, distinctly stated and elaborately defended. The Inquisition had power to imprison Galileo and commit copies of his work to the flames, but the truth it contained could not be burnt or bound. The earth 'still moved,' and matter continued to attract matter, unawed by the terrors of the Inquisition. The truth, once distinctly apprehended and announced, was never again to be lost, but was destined to grow in importance and be extended in its application far beyond the conceptions even of the great prophets of nature who were the first to proclaim it. The doctrine of Kepler on the subject of gravity may be regarded as,

historically, the foundation of that sublime superstructure which in a subsequent age was reared by Newton, and which, by reason of the magnitude of its proportions and the multiplicity of its details, all pervaded and determined by the most admirable unity, now stands and in all probability will ever stand, as the most imposing monument ever erected by the human intellect.

Although Kepler's theory, that bodies terrestrial mutually attracted each other, met with ready reception, more than thirty years elapsed after the publication of this work before the idea was entertained, at least favorably, of accounting for the revolutions of the heavenly bodies on the theory of the universality of the attraction of gravitation. Kepler indeed, as we have remarked above, alludes to such an hypothesis only however to expose, as he imagined, its fallacy. The motions of the heavenly bodies being curvilinear, whilst the motions of bodies under the influence of gravity were rectilinear, it was taken for granted as a thing self-evident that the two phenomena must be due to entirely different physical causes. Familiar as we are with the fact, that by the two laws of motion above mentioned, the hypothesis of an attractive force of the sun, combined with the hypothesis of a tendency of the planets to move in a straight line in virtue of an original impulse communicated to them, would satisfactorily and readily account for their curvilinear motion, it cannot but be a matter of surprise that the truth should have remained so long unrecognized.

The credit of having been the first to generalize the idea of gravity, and refer the revolutions of the heavenly bodies to the attraction of matter for matter, appears to be due to Borelli, an Italian philosopher, a pupil of Galileo. It is announced in a work which he published 'On the Satellites of Jupiter,' in 1666, although, as we shall have occasion to notice subsequently, Newton had conceived the same idea at least as early as 1665. Both Newton and Huyghens, however, attributed to Borelli the honor of having been the first to announce the important truth.

The idea, having been suggested, was at once accepted by many with favor and immediately led to the investigation of a hitherto unexplored field in the department of mechanical philosophy. Whilst the labors of others in this field were not unimportant, particularly those of Wallis, the name which is especially deserving of honorable mention in this connection is that of Huyghens. In a work published in 1672, we meet for the first time with a scientific discussion of *the doctrine of Central Forces*. His investigations were remarkably satisfactory and complete as to the phenomena of *circular* motion, the attractive force being at the center and contributing largely to the success of the labors of subsequent inquirers.

A great step had been taken toward the solution of the problem of planetary motion, but a formidable difficulty yet remained to be overcome. The orbits of the planets were not circular, but elliptical, and the sun—the center of the attractive force—was not at the center of the ellipse, but at one of the foci. For the complete solution of the actual problem which the phenomena presented, a calculus was needed which neither Borelli nor Huyghens possessed, and the preeminent genius of Newton was illustrated, probably more by the invention of the needed calculus than by his successful application of it to the solution of the important problem in question.

The general fact having been established that the curvilinear motion of the heavenly bodies was explicable on the hypothesis of a central attractive force, it was soon surmised that the particular character of the planetary orbits—involving as it did a continual variation in the distance of each planet from the sun, as well as a continual variation in the velocity of the planet's motion—could be due to no other cause than a difference in the intensity of the sun's attractive force at different distances. The query was: What was the precise law of this variation in intensity, which would account for the phenomena? Was the attraction inversely as the distance? or, as the square of the distance? or, as the cube? or, was it such as admitted of any precise expression? Guided probably by the best known fact as to the distribution of light, of heat, indeed of any emanation radiating in all directions from a center, several individuals, independently as it would seem, adopted the conclusion which was afterwards demonstrated to be correct, namely: That the attractive force of matter for matter varied inversely as the *square* of the distance, that is, at double the distance the attraction is one-fourth, at treble the distance one-ninth, and so on. The first to announce the true law of variation in the intensity of attraction was a French philosopher, Bouilland, or as his name ordinarily appears in the Latinized form, Bullialdus. About the same time, Sir Christopher Wren, the distinguished architect of St. Paul's, Dr. Hooke, for a long time secretary of the Royal Society, and the eminent mathematician astronomer, Halley, had arrived at the same conclusion. It was still however but a conjecture. In spite of the most earnest and persevering effort no one was able to furnish a demonstration.

As contributing to the discovery of the demonstration, the place of merit next to that of Newton, though of course far inferior, is doubtless due to Hooke. His labors were probably of aid to Newton by way of suggestion, without however affording any just ground for the charge which Hooke subsequently made that Newton was wearing the laurels to which he himself was justly entitled. As early as 1666 Hooke exhibited in the presence of the Royal Society an experiment

now quite familiar but at this time new and of exceeding interest. He suspended from the ceiling a long wire to the end of which a ball of wood was attached—a simple pendulum on a large scale. On removing the pendulum from the vertical position and then giving it a lateral impulse at right angles to the plane in which it tended to oscillate, the ball described an ellipse—the eccentricity of the ellipse varying with a variation of the intensity of the lateral impulse. An ocular demonstration was thus given of the important fact that elliptical motion could be produced by the combined action of two forces—one impulsive and the other central—and that the particular form of the ellipse depended upon the relative intensities of the two forces. Although in the experiment the attractive force was at the center of the ellipse, whilst in the case of planetary motion it was at one of the foci, still the fact exhibited must have been highly suggestive to any subsequent inquirer as to the cause of planetary motion.

In 1674 Hooke published a dissertation entitled 'An Attempt to prove the motion of the earth by observations,' in which he says: "I shall hereafter explain a system to the world, differing in many particulars from any yet known, depending upon three suppositions." The first—which he gave at some length—is a distinct statement of the universality of the attraction of gravitation. The second is substantially Kepler's law of inertia. The third is "that the attractive powers of the heavenly bodies are so much the more powerful, by how much nearer the body wrought upon is to their own centers." And, he adds, "Now what these several degrees are, I have not yet experimentally verified, but it is a notion which, if fully prosecuted, as it ought to be, will mightily assist the astronomers to reduce all celestial motions to a certain rule, which, I doubt not, will never be done without it." From this declaration it is evident, first, that at this time he was still in doubt as to the true law of gravitation; and second, that he was endeavoring to discover it by *experiment*—a method by which he could never have arrived at the truth. A few years later, as appears from his correspondence with Newton, Wren and Halley, he was fully convinced that the intensity of the attraction of gravitation was inversely as the square of the distance, and he even professed to be able to furnish a demonstration. In this he was either insincere at the time or discovered subsequently that his supposed demonstration was defective, as he never presented it, though repeatedly urged by Wren and Halley to do so.

We are now prepared to understand and appreciate aright the precise work which Newton performed in connection with the discovery of the law of gravitation. Born on Christmas day of the year 1642, the year in which Galileo died, in 1665 we find Newton a student of Trinity College, Cambridge, which he had entered in 1660. But

twenty-three years of age, he had already not only mastered all of value that had previously been written on Mathematics, Astronomy and Natural Philosophy, but he had discovered the Binomial Theorem, and had conceived and to an extent developed the Differential Calculus —an achievement with which few other events in the history of science deserve to be compared, after we except his own subsequent brilliant discoveries in Optics, and his successful application of the calculus to the discovery of the law and explanation of many of the most interesting phenomena of gravitation. In the summer of 1665 he left Cambridge on account of the plague which prevailed there at the time and returned to his native town of Woolsthorpe in Lancashire. It was during this visit to Woolsthorpe that the famous incident occurred which, as is generally supposed, first suggested to him the idea of gravitation and was the occasion of his great discovery. The account of it is given by his contemporary and friend Pemberton. One day as he was sitting under an apple tree in the garden an apple fell before him. This turned the currents of his thoughts and led him to reflect upon the nature of that mysterious influence which urges all terrestrial bodies toward the center of the earth, causing them, when free to move, to fall with a constantly accelerated velocity, which continues to act moreover without sensible diminution in intensity at the top of the highest towers or even the summit of the loftiest mountain. The thought was suggested to his mind, why may not this power extend to the moon? And if so, is not this the influence which retains her in her orbit round the earth? He at once applied himself to the determination if possible of the truth of this conjecture. If the moon were really retained in her orbit by terrestrial gravity, he concluded that the planets were probably retained in their orbits by a similar influence of the sun. Moreover, if the attractive influence of the earth extended to the moon and that of the sun to the farthest limits of our system, he concluded that the intensity of the attraction in each case diminished as the distance from the center of attraction increased. If this were so, it would manifest itself by a difference in the velocities of the planets, they being at different distances from the sun, and he accordingly inferred that by a comparison of the velocities of the motions of the several planets with each other, the law of variation of the intensity of the attractive force might be determined. Kepler's third law, that the squares of the times are as the cubes of the mean distances, furnished him at once with the necessary data for the calculation. He was not at the time able to solve the precise problem which the actual phenomena presented, the planetary orbits being elliptical and the attractive force at one of the foci, but assuming the orbits to be circular and the attractive force at the center, he found that Kepler's law would follow, if the variation in the

intensity of the attraction were *inversely as the square of the distance.*

It deserves to be noticed, that to solve even this problem, Newton must at the time have been familiar with the doctrine of central forces, though Huyghens' work on that subject was not published until more than six years after.

Though the data which Newton assumed were not precisely those which the planetary systems presented, the result reached was highly interesting and calculated to encourage and direct further inquiry. The next question to be determined was, the law of the variation of the *earth's* attraction—Was this also inversely as the square of the distance? If so, the universality of the attraction of gravitation varying in intensity according to the law just mentioned, would be almost indubitable.

The method by which Newton undertook to determine the variation of the earth's attractive influence—so simple when once suggested—was entirely original with him, and is one, though but one, of the grounds for attributing to him preeminently the honor of the discovery of the law of gravitation. Hooke, and doubtless others, subsequently labored for years to determine whether the intensity of the earth's attraction diminished with an increase of the distance from the center, and if so, according to what law, and yet all their efforts were fruitless. Newton's method was simply this, assuming the supposed distance of the moon from the earth to be correct, the length of the entire orbit of the moon may be readily determined. Moreover, the time of a complete revolution of the moon about the earth being known, the arc which she describes in one minute of time becomes known. Regarding this arc, which differs but little from a straight line, as the diagonal of a parallelogram, by the parallelogram of forces one of the sides of this parallelogram would represent the distance which the moon actually falls toward the earth under the influence of the earth's attraction in one minute of time. The arc just mentioned being known, this distance, which is the versed sine of the arc, may be readily determined. A measure is thus obtained of the intensity, at the moon, of the earth's attraction. By comparing this with the intensity of the attraction at the surface of the earth, as indicated by the distance a body near the surface will fall in one minute, the law of the variation in the intensity may be determined. Upon making the necessary computations the result was not just that which Newton anticipated, or rather hoped for. The distance which the moon ought to have fallen in one minute, according to the hypothesis, was one-sixth greater than that which, as it appeared, she actually did fall. Most men would have regarded this discrepancy as of little account, and accepting the result as, for the time at least, a sufficiently accurate demonstration of the hypothesis, would at once have given it publicity.

Newton, however, though he could not but feel assured that the true law of gravitation was indicated in the result he had reached, with that singular reticence as to his labors and indifference to fame which were among the marked features of his character, not only did not publish his investigations but did not even in his correspondence with his friends allude to the subject. For more than thirteen years he does not appear to have made any further progress toward the solution of the problem of gravitation. Though his attention was doubtless at times directed to it, he was mainly occupied during this period with other scientific labors, particularly in investigating the phenomena of light, making many brilliant discoveries on this subject which, even if he had not subsequently discovered the law of gravitation, would have entitled him to a distinction among men of science scarcely inferior to that which is now awarded him.

In 1679, after Bouilland, Hooke, Wren, Halley and others had become well convinced of the true law of gravitation and yet were unable to furnish a demonstration of it, Newton was led to a renewed investigation of the subject. Hooke had for some time been investigating the motion of projectiles, and in a letter to Newton about this time asserted that a body acted on by an impulsive force and at the same time by an attractive force varying in intensity inversely as the square of the distance, would describe an ellipse. What proof Hooke had of the fact asserted does not appear. It may be regarded as certain that he was not able to give a mathematical demonstration of it. As he had become well convinced that the attraction of gravitation varied according to the law mentioned, it is altogether probable that the main if not the sole ground for his assertion, was the fact that the orbits of the planets are elliptical. However this may be, Newton at once appreciated the importance of the assertion if it could be demonstrated, and was led to attempt the solution of the problem suggested by Hooke, or rather the converse problem, namely, to determine the law of variation in intensity of a central force which would cause the body acted upon to describe an ellipse. By the aid of the calculus, which he had by this time considerably perfected, he finally succeeded, after long and laborious effort, in demonstrating in its most general form the truth of Hooke's assertion. The importance of the result cannot be over estimated. The enigma which the elliptical orbits of the planets had presented was solved, and not only the fact of the sun's attraction but the precise law of the variation in intensity of that attraction was at last established beyond the possibility of further doubt or questioning.

The demonstration of the universality of gravitation however was still incomplete. The sun indeed attracted the planets with a force varying inversely as the square of the distance, but was this a property

common to all matter? Was it identical with the attraction of the earth which caused bodies near it to fall toward its center? Were the revolutions of the planets and the revolution of the moon phenomena referable to one and the same great law of nature? The result which Newton had reached in his investigations in 1665 seemed to render this doubtful, or at least presented a difficulty for the time inexplicable. Accordingly, with that characteristic reticence to which we have previously referred, Newton refrained from communicating to any one the important discovery he had made, preferring to await the solution of the difficulty which the anomalous fact of the apparent intensity of the earth's attraction at the moon presented.

Three years afterwards, in June, 1682, Newton attended a meeting of the Royal Society. Whilst in London, he accidentally learned that Picard in France had just measured the arc of the meridian with great accuracy, and that the result which he obtained for the length of a degree in that latitude differed somewhat from the measurement previously accepted as reliable. Newton at once perceived the importance of this fact in connection with the determination of the intensity of the earth's attraction. If the commonly received measure of a degree of the meridian was erroneous, the accepted estimate of the size of the earth was erroneous; moreover, if the assumed semi-diameter of the earth was incorrect, the supposed distance of the moon from the earth, in the calculation of which the earth's semi-diameter is involved, must also be incorrect. The possible explanation of the annoying result he had reached in 1665 was immediately suggested. Obtaining accurately the measurement of a degree of the meridian as given by Picard, immediately on his return to Cambridge he determined the size of the earth and the distance of the moon, on the supposition that Picard's measurement was the true one. With the data thus obtained he returned to the problem at which he had labored sixteen years before, and by the same method then pursued sought anew to determine the law of the variation of the earth's attraction. Perceiving, as he advanced in the calculation, the tendency of the numbers to produce the desired result, he became so much agitated that he was unable to finish the computation and was under the necessity of requesting a friend to do it for him. The identity of the force which causes bodies near the earth to fall toward its center and that which causes the heavenly bodies to revolve was fully established, the universality of the law of gravitation was finally and forever demonstrated, the solution of the grand problem of the universe was complete.

We might have supposed that Newton would have eagerly hastened to announce his great discovery and secure for himself the eminent honor to which he was entitled, and yet more than two years elapsed before the discovery was published to the world; and then, not of his

own motion but at the instance of his friend, Halley, who subsequently boasted that he was the Ulysses who had discovered Achilles and brought him forth from his concealment. In the month of August, 1684, Halley, having become satisfied that Hooke could not furnish the demonstration of the law of gravitation which he had repeatedly promised, visited Cambridge to confer with Newton on the subject on which he had become deeply interested and been long laboring without any satisfactory result. He inquired of Newton, what would be the curve described by the planets on the supposition that the attractive influence of the sun diminished as the square of the distance? Newton at once replied, 'An ellipse.' When asked how he knew this, he replied: 'I have calculated it.' Halley, surprised and delighted at the announcement, asked to see the demonstration. Newton was unable to lay his hands on the calculation he had made two years before, nor could he at the moment reproduce it. He promised Halley however that he would send him the demonstration as soon as he was able, and in the month of November following he fulfilled his promise. Halley immediately revisited Cambridge to obtain Newton's consent to the publication of the discovery. In this he succeeded, and on the 10th of December he informed the Royal Society of the discovery and that Newton had consented to prepare a paper on the subject for the society. In February, 1685, the promised communication was received—a paper of twenty-four pages, containing four theorems and seven problems. He refers to it in the accompanying letter, as his 'Notions about Motion.' This humble yet most memorable paper ever presented to the Society was the germ of 'The Principia.'

The great discovery having been made public, Newton seems to have felt that the time had come to enter on the gigantic task he had doubtless proposed to himself when the discovery was first made but which other occupations had hitherto prevented him undertaking, namely, putting his demonstration in a complete and conclusive form and applying it to the solution of the many interesting and sublime problems which the phenomena of the material universe presented. For two years he dismissed from his mind all other occupation, and devoted himself with all the energy of his mighty intellect to the Herculean task. With untiring industry, prolonged attention, and intense thought, probably never paralleled in the history of intellectual effort, he lived but to meditate and calculate; oftentimes so wholly absorbed with the grand themes which occupied his mind as to be for the time unconscious of all the ordinary concerns of life. Frequently, on rising in the morning he would sit for hours on his bedside arrested by some new conception, and had it not been for the attention of the members of his family would often have neglected to take his daily food.

We cannot enter upon any detail or present even a summary of the magnificent result of these labors. They are to be found in his immortal work, the 'Philosophiæ Naturalis Principia Mathematica,' given to the world in 1687 under the auspices of the Royal Society. Of this work, the great Laplace, who, of those who have applied the highest powers of the human mind to the investigation of the phenomena of gravitation, stands second only because Newton lived before him, says: "The universality and generality of the discoveries it contains, the number of profound and original views respecting the system of the universe it presents, and all presented with so much elegance, will insure to it a lasting preeminence over all other productions of the human mind." "It is a work," says Sir David Brewster, "which will be memorable, not in the annals of one science or one country only, but which will form an epoch in the history of the world, and will ever be regarded as the brightest page in the records of human reason. It is a work which would be read with delight in every planet of our system, and in every system in the universe. There was but one earth on whose form and movements and tides the philosopher could exercise his genius; one moon whose perturbations and inequalities and action he could study; one sun whose controlling force and apparent motions he could calculate and determine; one system of comets whose eccentric paths he could explore and rectify; one universe of stars to whose binary and multiple combinations he could extend the law of gravity. To have been the chosen sage, summoned to the study of that earth, these systems and that universe, the favored lawgiver to worlds unnumbered, the high-priest in the temple of boundless space, was a privilege that could be granted to but one member of the human family; and to have executed the task, was an achievement which, in its magnitude, can be measured only by the infinite in space, and in the duration of its triumphs by the infinite in time."

PLANTS AS WATER-CARRIERS.

By Professor BYRON D. HALSTED,
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A GIANT redwood, the monarch of the California forests, stands with its stem-tip three hundred and fifty feet above the soil. From the surface of the millions of tender delicate leaves near the top of the tree there are exhaled many gallons, perhaps barrels, of water daily. The force required to make good this loss is, of course, equal to that needed to raise the water through the three hundred feet or more of vertical space. It is no wonder that the thoughtful person will pause as he contemplates this exhibition of force. It makes no noise; work is being done, but it is not easy to see how.

Let us begin with the soil, as that is the source of the water supply of plants, and briefly consider its constitution, texture and relations to the problem of water-carrying. In other words, does soil carry water and, if so, in what way is it conveyed? Soil is rock that has been broken into small pieces in one way or other, a refinement of rock, so to speak, whether by frost, moving water or chemical action. For our purpose soils, having many degrees of fineness, may be classified into coarse, medium and fine. Coarse soil may be compared to masses of cannon balls touching each other but with large spaces between them, while the medium soil is similar to peas in piles, and the fine soil is like clover seed. The chief difference is in the amount of surface exposed by the particles which go to make up a definite portion of soil.

The next point for us to consider is the capacity of soil for holding moisture. Thrust the hand into a dish of water and upon removal it will be wet, except any portion that has been coated with oil or similar substance. In short, water will leave its own mass and adhere to the surface of the hand. If the hand held a quantity of clean earth the latter would likewise become wet. The amount of water that the soil will hold depends upon the surface exposure of its particles. As this is an exceedingly important point, permit me, at the risk of dealing largely in dry figures but for explanation and proof, to draw upon some results given by Professor King in his charming book 'The Soil.' With columns of sand ten feet long, the one with the grains averaging in diameter 186/10,000 inch, after percolating for 111 days, contained 3.77 per cent. of water; the 73/10,000-inch grains retained 4.92 per cent.; the 61/10,000-inch grains, 5.76 per cent. and the 45/10,000-inch grains, 7.57 per cent. In other words, the smaller the size of the

grains and the pores the greater the amount of water retained. With soils of still smaller particles the water-holding power would be correspondingly greater.

If the soils are placed in long upright glass tubes and water is added at the bottom, it will rise through the soil to the top. This phenomenon of capillarity is best illustrated with tubes having bores of varying diameter. The tables given us on this subject are to the effect that, when an inch tube is plunged into a vessel of water, the height of the water column in the tube above the general level is .054 inch; for a 1/10-inch bore .545 inch, 1/100-inch bore 5.456 inches and for a 1/1,000-inch bore, 54.56 inches. While the actual surface pull of the smallest tube is much less than that of the largest, it is through a vastly greater distance and by a multiplication of the number of such minute tubes in a given space that the greater lifting is brought about.

The soil itself, consisting of minute particles, admits of the capillary action; for the pores, although not straight, extend in irregular lines and permit the surface tension that is evident in fine tubes. This lifting power of minute passageways is abundantly illustrated in the everyday operations of crop-growing, and the skilful tiller makes abundant use of it or checks it as best suits his purpose. If a dark soil contains an abundance of light alkaline salt, it is possible that it may have a white crust form upon the surface during a drought to be carried back upon the falling of a substantial rain, and this rise and fall may be repeated indefinitely as happens on some of our alkaline lands, where the precipitation is light and vegetation scant.

It has been shown that the soil, on account of its porosity, is able to lift water through considerable distances, simply through the greater pull of a solid for the liquid than the liquid has for its own particles. The hand is wet by the water; a towel hung high with barely one corner dipping into the basin may become wet throughout, and, by evaporation, the dish may be pumped dry.

Into this complex physical porous mixture, to the component particles of which a liquid adheres with such force as to be present when even the air is dry, the plants establish themselves by means of their tiny rootlets and the much more minute root hairs which, insinuating themselves between the microscopic pebbles, become misshapen and contorted beyond all recognition of the simple vegetable cells out of which they have grown. The movements of the water in the soil, whether to the right or left, up or down, are governed, as has been shown, by the law of surface attraction. When we come to the plant cell, the whole physical basis is changed, and, among other things, we are brought face to face with membranes of extreme thinness and delicacy, and, more than all, with the living protoplasmic film.

The two ends of the microscopic aqueous canals, in plants of the ordinary sort, are the active imbibing root-hairs, above mentioned, and the exposed surfaces of the aerial parts, chief of which are the leaves. The lower terminal is buried beneath the soil from which it receives its supply, and consists of a sac with a very thin and elastic wall, lined with a delicate film of living protoplasmic substance. We may imagine one of these cells many hundred times enlarged, its contents consisting of a thin syrup, but slightly more dense than the liquid in which it is immersed. As time passes, there is a flow inward of the less dense liquid and an increase of the wall tension of the cell. This tension might be observed by pricking the wall, when from the pin-hole the liquid would spurt for some distance. The same pressure might aid in the passage of the liquid from the sac to the one next adjoining, and in that way a flow would be set up from the less to the more dense cell contents.

A homely and common illustration of this osmosis or membrane diffusion is seen in the action of sugar upon ripe strawberries, the sugar taking the thin juice from the cells and making a syrup that finally surrounds the berries. Place dried prunes in a dish of warm water, and a similar exchange is demonstrated, but in this case the flow is into the dry cell contents, and the prunes finally become plump. There has been a transportation of liquid in both these instances, and it has been from one cell to another through the whole necklace, so to say, of many beads, from the surface to the innermost cell or *vice versa,* as the case may be.

Let us ascend a tall tree, figuratively, and study microscopically the upper terminals of the lines of water-carriers. Here we find the leaves in great numbers, presenting, possibly, acres of actual visible surface to the drying influence of the almost constantly changing air. But if we note the exceedingly porous structure of a leaf, how one cell touches its fellows at but few points and the bulk of the space is intercellular, the actual surface exposed to the atmosphere is a hundred times more than the naked eye reveals. As with the soil terminal, so here the end of the transportation line divide up into a million parts. In the former, each is for the reception of liquid; in the latter, they are all places of unloading. The drying air sweeps over them, and something of their contents is vaporized and is gone from the plant. But this evaporation increases the density of the cell contents and were there no reserve the tissues would wither, dry up and become dead, as is the case when a branch is cut off or grass is mown in the meadow.

If we apply the law illustrated in the dried prunes, it will be seen that each surface cell in the loose pulp of the leaf is dependent upon the next below it, and that, in honoring the draft upon it, is making a physical call upon another, and so the line is established, like men

passing buckets at a fire, or tossing melons in the loading of a schooner for a northern market.

The whole story of water-carrying is not ended with the above. One of the most delicate of all plant mechanisms is that which is associated with the transportation of its liquids. The leaves and green surfaces generally are closely studded with minute structures, 100,000 or more to a square inch, that open or close as the emergencies of the case demand. They are vitalized and exceedingly sensitive valves, usually constructed of two crescent-shaped cells set in the skin and highly charged with protoplasm. These organs are influenced by sunlight and darkness, by heat and cold; in fact, their functioning calls forth the admiration of any careful student of the subject. The two guard cells are so hung that they become turgid when the leaf is well filled with water, and thus enlarge the opening to its full capacity for the passage of vapor-laden gases. As soon as these guard cells lose much water, they become less plump, and this brings about the closing of the pore. They are, therefore, valves of safety, and, as the other portions of the leaf are covered with a cuticle more or less impervious to gases, it is seen that the stomates are the organs that regulate the evaporation stream.

That the amount of water carried is very great scarcely needs to be emphasized. Note the rapidity with which grass wilts when cut for hay or the leaves upon a branch that has received any injury. If a melon vine with twelve leaves will carry a liter of water in a single day, as it has been known to do, what must be the vastness of the lift in a forest of a thousand acres upon a dry day when the leaves are fresh and most active!

That it needs to be great is seen from the requirements of the plant. The soil water is weak in all salts that a plant must acquire, and to take them in concentrated form would be as poison. The whole plan, therefore, is to carry large quantities of a dilute solution, and afterwards bring it to the required strength. In the evaporation there is a cooling obtained that may possibly save the plant from destruction.

We thus far have seen that an ordinary plant has its slender, delicate, insinuating root-hairs closely applied to the soil particles from which they imbibe the adhering moisture. It has further been shown that the opposite terminal of the waterways has also a vast number of delicate living cells exposed, not dangerously, to the drying action of the atmosphere. Between these two extremities is the body of the tree, the main roots and branches, and it is for us to determine through what parts the upward flow takes place. This admits of demonstration by the removal of certain portions and observing the effects. That it does not take place through the central or heart wood is to be expected, for the cells here are often all filled up with lignin and coloring

matter, and the way is blocked; the canal is filled with débris, so to say, and has become disused. Again, the old central wood frequently decays until there is only the outer ring of the later-formed wood remaining with the bark that covers it. That the bark is not the water-carrier may be shown by removing a ring of it and thus breaking the connection without interrupting the upward flow. That it does pass through the young wood may be shown by cutting this portion without harming materially the bark or the heart wood, when the leaves quickly wither and the tree may die. In short, the sap-wood is well named, as through it the soil-water mounts upward from the roots to the leaves.

In many plants, however, there is no well-developed ring of wood. Either the stem is too young to have one or its construction such that it never appears. However, the same kind of tissue is somewhere to be found in the stem, usually in strands or portions of tough threads, as in the corn-stalk, and through these the crude sap is transported. Some of these succulent stems are so transparent that they admit of experiments which demonstrate both the path and the rate of the upward flow. For example, a balsam stem may be cut and, while fresh, plunged into a harmless colored liquid, as that of some aniline dye. It is found that the woody bundles are the first to take the stain and that it mounts upward with a rate that is an index of the flow of sap and may be some feet in a single hour. Another test for the rate is found in the use of a harmless salt, easily detected in extremely minute quantities by the spectroscope. Let it be lithium nitrate, for example, and its rise discovered by making sections of the stem at different distances and burning small fragments.

But having determined the place of entrance, line of ascent and point of departure of the aqueous stream, it by no means follows that all the forces have been named that bring about the transfer. That living plants carry water and make it one of the chief labors of all their active days is beyond question, but physicists and physiologists, chemists and biologists are as one concerning the mystery that here exists. A grape-vine stump bleeding in early spring is a stumbling block for them all, and they fall back upon 'root pressure,' a term more convenient for covering much ignorance than for service as a full, well-rounded explanation of the phenomena in question. Membrane diffusion will account for much, capillary attraction helps considerably, and the differences of gas pressure within and without the cells, as in the tapped sugar maple in early spring, count for something; but back of all is a vital force that has not been reduced to a physical or chemical basis.

THE SOLUBLE FERMENTS OR ENZYMES.*

By Professor EDWIN O. JORDAN,
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IT has been said somewhat sententiously that the advance of science consists simply in a change of problems; we achieve progress when we substitute for one problem another at once more delicate and more precise. The recent history of the theory of alcoholic fermentation furnishes a conspicuous illustration of this aphorism. Liebig's ingenious conception concerning the breaking-down of the sugar molecule by the decomposing albuminous compounds in dead and dying yeast cells—his notion being that the sugar is toppled over, so to speak, by the mechanical shock of other falling molecules—was forced to yield to Pasteur's apparently clear demonstration of the part played under natural conditions by the living yeast cell. More recently the conception of the living cell as the essential feature of the process has been dethroned in its turn, and alcoholic fermentation is now shown to rest on the action of an 'unorganized,' 'lifeless,' or 'soluble' ferment or enzyme, secreted by the yeast plant. Further, the action of this enzyme and of other and more familiar 'unorganized' ferments has been brought into line with some of the most characteristic activities of the living cell, and many general life phenomena have been shown to be in reality phenomena of fermentation.

The consequent focusing of attention upon the enzymes, their nature, mode of action and chemical relations, has already been prolific in results of great biological interest. The science of experimental medicine, in particular, is discovering that many of the problems relating to immunity, to toxins, antitoxins and agglutinating substances are closely connected with the problems of fermentation and the enzymes.

So far as is known, the peculiar substances called enzymes are produced only by the living cells of animals and plants, although there are certain inorganic substances that so closely reproduce the essential qualities of enzyme action that they might almost be termed 'inorganic enzymes.' The enzymes obtained from the animal or plant cell have

* *The Soluble Ferments and Fermentation.* J. Reynolds Green. Cambridge University Press, 1899.
Masson, Paris, 1899.
Traité de microbiologie, II., diastases, toxines et venins. E. Duclaux.
Les enzymes et leurs applications. J. Effront. Carré et Naud, Paris, 1899.

never been isolated in a pure condition, and hence no definite knowledge of their chemical composition and constitution has yet been secured. In general, enzymes are precipitated by alcohol from the watery extract of plant or animal tissue, but such a precipitate contains the enzyme inextricably mingled with other chemical compounds, and all attempts to separate out a pure enzyme have thus far signally failed. The whole field of enzyme study is therefore at present beset with the same sort of difficulties that the science of bacteriology encountered before the days of 'pure cultures,' and it is doubtless true that effects that are at the present time ascribed to individual enzymes are in reality caused by mixtures of distinct varieties.

The different kinds of enzymes are at present chiefly distinguished through the differences in the changes that they produce in other substances. The enzymes that act upon starchy substances, for instance, may be conveniently grouped together; those that disintegrate albuminous compounds may be similarly treated, and so on.

Enzymes converting starchy substances into sugar. The longest known and perhaps most thoroughly studied enzyme is a representative of the group that transforms insoluble carbohydrate substances into soluble ones. *Amylase,* or *diastase,* is the well-known enzyme that accomplishes the conversion of the starch of the barley-grain into sugar in the process of malting. The action of the enzyme in this process appears to be quite elaborate since the complex starch molecule passes through several stages during its conversion into sugar, the hardly less complex substances known as 'erythrodextrins' and 'achroödextrins' being formed on the way. The theory has been advanced that the starch molecule breaks down by the taking on of successive molecules of water and by subsequent decompositions, sugar (maltose) being formed at each splitting, together with a dextrin of lower molecular weight. Duclaux, however, maintains the existence of two enzymes in malt, one, a liquefying or decoagulating enzyme to which he would restrict the name amylase, and which converts the insoluble starch into the soluble dextrins, and a second (dextrinase), which has a saccharifying power and converts the dextrins into sugar. Other enzymes that may be placed in the same group with amylase are *inulase* and *cytase.* Inulase converts into fruit sugar a reserve food-substance found in many plants and known as inulin. Inulin is allied to starch in its chemical composition, and probably breaks down by successive stages under the action of inulase just as starch does under the influence of amylase. Another form of reserve food-substance stored up by many plants is the familiar substance comprising the cell-wall of most plants and known as cellulose. This substance, like inulin and starch, can be changed into a more directly utilizable substance by the action of *cytase,* an enzyme found

in many plant cells. The products of the activity of cytase have as yet been imperfectly studied, and it is possible that several different enzymes, corresponding to the different kinds of cellulose, are at present included under this name. Cytase may be useful in various ways to the organisms secreting it. Some parasitic fungi are able to attack and penetrate the cell-walls of the host-plant by virtue of the dissolving power of the cytase secreted in the tip of the growing hyphæ.

Enzymes acting upon sugars. Another group of enzymes is concerned with the conversion of the higher sugars or polysaccharides into the lower. The classic instance is the so-called 'inversion' of cane-sugar or sucrose into equal parts of grape-sugar and fruit-sugar, according to the equation:

$$\underset{\text{sucrose or cane-sugar.}}{C_{12}H_{22}O_{11}} + H_2O = \underset{\text{dextrose or grape-sugar.}}{C_6H_{12}O_6} + \underset{\text{levulose or fruit-sugar.}}{C_6H_{12}O_6}$$

A solution of cane-sugar turns a ray of polarized light to the right, but the mixture of dextrose and levulose, owing to the superior lævorotatory power of levulose, turns the ray to the left, whence the term 'inversion' as applied to this process. The enzyme that is able to produce the inversion of cane-sugar was discovered by Berthelot in 1860. *Sucrase,* or *invertase,* is found in many yeasts and other fungi, in pollen-grains, in the beet-root, and to a slight extent in some animal secretions. Cane-sugar is not directly assimilable by the animal body, and if injected into a vein is excreted almost unchanged. The inversion which occurs in the intestine when the cane-sugar is taken into the body by way of the alimentary tract seems to be a necessary preliminary to the utilization of this sugar as a food substance.

The same is true to a certain extent of maltose, which is a sugar of the same percentage composition as sucrose, but with a different arrangement of the atoms within the molecule. Maltose is split up by the action of the enzyme *maltase* into two molecules of dextrose. Maltose, like sucrose, is only with difficulty assimilable, and its conversion into dextrose constitutes an important phase of carbohydrate nutrition. Maltase, like sucrase, is a widely distributed enzyme and is found in many animal and plant tissues.

The action of maltase upon maltose presents a significant example of what chemists call a 'balanced' action. When a certain proportion of maltose has been converted into dextrose, the action of the maltase ceases, and if now an excess of dextrose be added the action of the enzyme is reversed, and a certain proportion of the dextrose is converted back into maltose until a new equilibrium be reached. This reversibility of action has been thought to indicate that the action of maltase falls in line with other chemical reactions and is not essentially different from that evinced by many well-studied inorganic substances.

Together with sucrase and maltase may be grouped other enzymes that split up the higher sugars into those of lower molecular weight, such as *lactase* which converts lactose or milk-sugar into dextrose and galactose, *trehalase* which splits up trehalose, a sugar obtained from Syrian manna, into two molecules of dextrose, and *raffinase* and *melizitase* which act upon certain of the higher polysaccharides.

Coagulating enzymes. The group of clotting or coagulating enzymes includes two comparatively well-known enzymes, *rennet* and *plasmase* or fibrin ferment. The use of rennet in setting curd for cheese and in preparing the delicate dessert known as junket is generally familiar. The source of most of the commercial rennet is the extract of the mucous membrane of the stomach of the calf; this enzyme is found also in many other young mammals during the period of lactation. Rennet has been obtained likewise from several vegetable sources; parts of the plant *Galium* are used in the country districts of England to aid in the formation of curd in cheese-making, and the peasants of the Italian Alps use the leaves of the butterwort (*Pinguicula*) for a similar purpose. The curdling or precipitation of the casein by rennet is singularly dependent upon the presence of salts of lime. A very minute quantity of rennet in the presence of calcium salts will curdle a prodigious quantity of casein. It is in fact uncertain whether rennet can act at all in the entire absence of calcium. In the presence of calcium the potency of rennet ranks higher than that of any other enzyme yet studied, one part of rennet being able to coagulate more than 250,000 times its own weight of casein.

The phenomenon of the clotting of blood is dependent upon a variety of factors as yet imperfectly understood. That the fibrin or solid portion of the clot is separated out from the blood plasma by the action of an enzyme is, however, solidly established. The character and mode of action of this enzyme—termed *plasmase,* or fibrin ferment—are still quite obscure, although the fact that in mammalian blood the enzyme originates from the leucocytes, or white blood corpuscles, seems to be generally admitted. In birds the enzyme exists in the cells of the tissues and not in the blood corpuscles. The blood of all vertebrates with nucleated red corpuscles presents a marked resistance to spontaneous coagulation; clotting, on the contrary, is almost immediate among the mammals, which possess enucleated red corpuscles. As is the case with rennet, calcium salts favor coagulation; their presence seems, however, not to be necessary.

Other clotting phenomena have been shown to be due to enzyme action. The formation of jelly from the juices of various fruits and berries is due to the gelatinizing or coagulating effect of an enzyme, *pectase,* which acts upon pectose, a carbohydrate allied to cellulose and occurring in many fruits and vegetables.

The various phenomena of clotting and gelatinizing belong in the debatable border land between physics and chemistry. Duclaux has given in his treatise a clear exposition of his reasons for believing that the changes involved in the various processes of coagulation are due to a disturbance of the physical equilibrium of the substances in solution rather than to any chemical reaction.

Enzymes acting upon proteid substances. One of the most important groups of enzymes is that of the proteolytic enzymes, characterized by their property of breaking down albuminous or proteid compounds into simpler ones. Owing to the prominent rôle they play in the human body in connection with digestive processes they have been subjected to exhaustive study. Two chief groups are recognized, the *peptic enzymes,* of which pepsin, the enzyme of the gastric juice, is the type, and the *tryptic enzymes,* the best known of which is the trypsin secreted by the mammalian pancreas. The peptic enzymes are almost unique among known enzymes, inasmuch as they can act only in an acid medium; they are further characterized by their inability to carry the decomposition of proteid substances beyond the 'peptone' stage. The tryptic enzymes, on the other hand, are most potent in a slightly alkaline medium, and they are able to push proteid decomposition to a point beyond that reached by the pentic enzymes. Two of the most characteristic end-products of tryptic digestion are the substances leucin and tryosin which, like urea, are not assimilable by the tissues and are eliminated from the body. Several tryptic enzymes of vegetable origin are known, among which *bromelin* from the juice of the pineapple and *papain* from the fruit of the papaw-tree have been thoroughly studied. It is probable that the digestive enzyme secreted by insectivorous plants belongs to the tryptic class.

An interesting tryptic enzyme has been recently discovered in fresh milk by Professors Babcock and Russell. This enzyme, called *galactase* by its discoverers, acts upon the proteids in milk and plays a most important part in the manufacture of cheese; it is probably responsible for many of the phenomena of cheese-ripening that were formerly ascribed to bacteria.

Owing to the great chemical complexity of proteid substances and to the fact that little is known about their chemical constitution, the study of proteolytic enzymes is hampered by difficulties of an especially serious nature. Since neither the initial composition of the proteid compound nor the substances to which it gives rise on decomposition can be accurately determined, the distinction of different kinds of proteolytic enzymes is attended with greater difficulty than is experienced in the case of enzymes that attack chemical compounds so comparatively well understood as the sugars.

The alcoholic enzyme. The enzyme that converts sugar into alcohol and carbon dioxide has been only recently discovered, although it has been diligently sought for since the time of Pasteur. The discovery of Buchner in 1896 that, by applying great pressure to a mass of yeast cells, an enzyme, which he named *zymase,* could be extracted gave in fact a new impulse to all enzyme study. Zymase appears to dislocate the sugar molecule according to the classic formula:

$$\underset{\text{dextrose}}{C_6H_{12}O_6} = \underset{\text{alcohol}}{2C_2H_6OH} + \underset{\text{carbon dioxide}}{2CO_2}$$

$$\underset{180\text{ gr.}}{\phantom{C_6H_{12}O_6}} \qquad \underset{92\text{ gr.}}{} \qquad \underset{88\text{ gr.}}{}$$

The explanation of the prolonged failure of investigators to discover zymase lies in the fact that this enzyme is closely associated with the substance of the living yeast cell and does not diffuse out into the surrounding medium as does another common yeast enzyme already mentioned under the name of invert-ferment or sucrase. In solution, zymase quickly loses its strength, probably partly because of oxidation, partly because of the destructive action of the tryptic enzymes of yeast. Zymase is able to convert a number of different sugars into alcohol and carbon dioxide: maltose and sucrose are readily fermentable, galactose much less readily and lactose not at all. Glycogen can be slowly fermented by zymase, but is not fermented by the living yeast cell because it can not pass through the cell-membrane into the cell and zymase can not pass out. The brilliant researches of Emil Fischer upon the relation of the configuration of the sugar molecule to its fermentability have demonstrated how delicate is the relation obtaining between the structure of the sugar molecule and the enzyme that attacks it. A slight rearrangement in the position of the atoms within the molecule, the actual number of atoms remaining all the while the same, is sufficient to determine whether a sugar can be fermented or not. Only in those cases where the geometrical build of the enzyme conforms to that of the sugar molecule can fermentation occur. To use Fischer's metaphor, the enzyme must fit the substance it attacks as closely as the right key fits the wards of the lock that it opens.

Other enzymes. A few other important enzymes can be but briefly mentioned, since the limits of this review do not permit of a fuller consideration. A group of enzymes of which *emulsin* is the type may be classed as the glucoside-splitting enzymes. These ferments are able to split up glucosides—which may be described as compounds of glucose (or some other sugar) with an alcohol, ether, aldehyde, or similar body—into glucose and the aldehyde or other associated compound. *Emulsin* is found in many plant tissues, but it is doubtful if it occurs in any animal body. The physiological rôle of emulsin is not wholly understood; it is possible that the glucose formed by the enzyme action is useful in the nutrition of the plant, or it may be true that the toxic

or bitter principle also split off has a protective value and prevents injury to the plant by animals. Besides emulsin, a few other glucoside-splitting enzymes are known.

Lipase, a fat-splitting enzyme; *laccase,* an oxidizing enzyme concerned in the production of the famous black varnish used in lacquer work; and *urease,* an enzyme that converts urea into ammonium carbonate, are among the other better-known enzymes.

There is reason to believe also that the various anti-microbic substances found in the bodies of some artificially immunized and some naturally immune animals are to be regarded as enzymes, as is likewise the substance that is found in the blood of typhoid patients and that has a 'clumping' or 'agglutinating' action upon the typhoid bacilli. Research in this direction has not, however, proceeded far enough to enable us to offer anything more than a conjecture as to the real character of the 'agglutinines' and 'lysines.'

The precise mode of action of enzymes has been the theme of much speculation. Perhaps the simplest and most natural view of some cases is to suppose that the enzyme combines first, for example, with a molecule of water, and then attaches itself to the body upon which it acts. This new compound, meeting with another molecule of the same substance, is then decomposed into the body which the enzyme produces and the enzyme itself. The enzyme thus acts as a simple intermediary, bringing the molecule of water or oxygen in closer contact with the fermentable substance. This view has certain arguments in its favor, as for instance the fact that the enzyme does not exhaust itself in the course of the changes that it produces. If it is unceasingly decomposed and reconstituted the reason for this is clear.

Such an explanation, however, is hardly valid for the action of zymase upon sugar and for the reversible action of maltase. Now there are certain facts regarding the action of mineral acids upon sugars and proteids, of various salts upon the phenomena of clotting and oxidation and of other changes brought about by inorganic substances which render it difficult to set enzyme action apart as a thing by itself. The action of an enzyme is essentially 'catalytic,' that is, it is able to exert an influence wholly out of proportion to its quantity, and itself remain unaltered at the end of the process. It has been pointed out that the influence of an enzyme or indeed any catalytic agent is simply to retard or accelerate changes which ordinarily take place more slowly or more rapidly. In other words, an enzyme simply influences the rate of change, not the final condition of the substance upon which it acts. The nature of the change, the final state of chemical equilibrium, is determined by the chemical forces within the substance itself, the speed at which the change occurs is determined by the enzyme.

DISCUSSION AND CORRESPONDENCE.

GEOLOGY AND THE DELUGE AGAIN.

To the Editor:—The August number of THE POPULAR SCIENCE MONTHLY contains a letter signed 'X. Plain' which asks Prof. G. Frederick Wright certain questions in regard to his recent article in 'McClure's Magazine' entitled 'Geology and the Deluge.' Is this quite a fair procedure on the part of X. Plain? He knows that Prof. Wright holds the chair of the harmony of science and religion in Oberlin College, and that he is filling the position in a manner satisfactory to his constituents. Then is it fair, I ask, for X. Plain, whoever he may be, to stand behind the door and, thus protected by an assumed name, take Professor Wright to task for doing his legitimate business in an acceptable, even masterly manner? X. Plain evidently does not dare to say what he thinks upon biblical matters. Nobody in this country does. Ingersoll said a few things, and I have heard good Christians say that he should have been burnt alive for saying them. Other very good Christians whom I know will not permit his works in their homes. Elbert Hubbard rather puts a damper upon the supposed harmony of science and biblical history by saying a few things every little while. Ingersoll was not afraid, and Hubbard is not afraid, but the rest of us are, X. Plain included. We do not dare to put in words our estimate of the Bible if we wish to retain our positions, either professionally or socially. The professor of biology today must teach evolution. The tide of evidence is so overpowering that he is carried *nolens volens* along with it. The church has been forced to accept the 'theory' of evolution. It has 'harmonized' apparent discrepancies, and has comforted trembling souls as mothers do their little ones: "There, there, there, Evolution shall not hurt you." But a professor of any one of the natural sciences is obliged to be a very juggler with words, in order to teach the truths of evolution to those who are able to comprehend them, and at the same time not disturb the faith of those who wish to keep intact the religion, or rather theology, which they have inherited. Because of that imperative law of evolution, self-preservation, we must earn our bread and butter. Therefore the myths of Genesis are 'reconciled' with science. Hence we fall meekly into line, and, as we dare not express our thoughts freely, we maintain a careful silence. In melancholy proof of which, I too must use the shield of anonymity.

Y. OBSCURE.

SCIENTIFIC LITERATURE.

ANTARCTICA.

THE great national antarctic expeditions of Great Britain and Germany are now on their way to the southern hemisphere, the *Discovery* having set sail from Cowes on August 6, and the *Gauss* from Kiel on August 11. Those who wish to follow intelligently the results of the explorations of the next three years will want to know what has been already accomplished, and there are fortunately two books giving the necessary information. A little while since, The Macmillan Company published in America a translation by Mr. A. Sonnenschein of Dr. Karl Fricker's excellent work on the 'Antarctic Regions.' With true German thoroughness, he begins with the conjectures of Aristotle and follows the history of discovery to the recent expeditions. The book is elaborately illustrated with maps and photographs. Still more important is the 'Antarctic Manual' prepared for the use of the British expedition. Under the auspices of the Royal Geographical Society and through the initiative of Sir Clements Markham, Dr. George Murray, the director of the civilian scientific staff, has compiled a volume of 600 pages, giving information likely to be of use in the conduct of the expedition. The contributors are the most eminent British men of science. Lord Kelvin writes on atmospheric electricity and Professor Schuster on the aurora; Professor Darwin on tidal observations and Professor Glazebrook on the pendulum. The contributors on the natural sciences include Professors Bonney and Gregory, and Messrs. Lydekker, Boulenger, Fletcher and Murray. Then a number of older articles are reprinted, the 'Narrative' of Charles Wilkes, the 'Journal' of Dumont d'Urville, etc. The work has been prepared for the officers of the expedition, but a limited edition will be sold to the public through Mr. John Murray.

INVENTIONS.

'TWENTIETH CENTURY INVENTIONS; A Forecast,' by George Sunderland (Longmans, Green & Company) is a readable, sober and profitable book which is entirely free from the exaggerations which mark most efforts in this direction. The author takes the ground that the germs of future inventions are already formed and that future progress is but the evolution of present tendencies. Thus he develops a new form of steam engine from the principles of the construction of the aneroid barometer, a new form of railroad from the rope cableway, and a new method of typesetting from the linotype machine. It is a curious fact that some of his proposed methods have not only been invented, but actually used in this country; for instance, the inclined movable staircase, electric motors for house elevators, electric heating, and the generation of power by wave action have already been shown to be possible if not feasible economically. The author is sound in his general ideas of evolution, but it may be possible that the century will witness inventions which he and we cannot imagine.

THE PROGRESS OF SCIENCE.

THE DENVER MEETING OF THE AMERICAN ASSOCIATION.

THE chief scientific event of August is the annual meeting of the American Association for the Advancement of Science, and the present meeting is of more than usual significance. It is doubtless a mere coincidence that the fiftieth meeting of the Association and the first meeting of the twentieth century should be the first to be held in the western states. The meeting itself is, however, nearly as important an event for science in the west as was the original foundation of the Association for science in the east. It means that the scientific men of the western states have now become sufficiently numerous and influential to meet on terms of equality with those of the east. The development of scientific work in the central and western states during the past ten years has perhaps never been rivaled in the history of civilization. Of the twelve American universities having in their faculties the largest number of scientific men, seven are in this region—Chicago, California, Michigan, Minnesota, Wisconsin, Illinois and Stanford. Each of these universities has on its faculties twenty-five or more scientific men, apart from medicine and engineering, and other institutions —Nebraska, Kansas, Missouri, Iowa, Indiana, Texas, Washington and more —will soon be of the same rank. With a prejudice that is not unreasonable, we assume that the scientific intelligence of the country may be measured by the percentage of people that subscribe to this journal. Massachusetts would, by this criterion, stand first, but Colorado would have twice the intelligence of New Jersey, California nearly three times the intelligence of Pennsylvania and Arizona ten times the intelligence of Maryland. During the past ten years the population of the western half of the country has not increased appreciably more rapidly than that of the eastern half, but its educational and scientific development has been truly marvelous.

THE meeting of the American Association at Denver, midway between Chicago and the Pacific coast, will be largely attended by those scientific men for whom it is the geographical center, and the excursion to Colorado is so attractive that the eastern states are certain to be well represented. The council holds a preliminary meeting on August 24, but the meeting really opens on the twenty-sixth. In the morning there is the usual formal welcome by the governor of the state, the mayor of the city and other officers, and the presidency is transferred by Professor Woodward, of Columbia, to Professor Minot, of Harvard. On Monday afternoon the addresses of the vice-presidents are delivered, and on Tuesday the retiring president gives his address, the subject being 'The Progress of Science.' During the week the Association meets in nine sections, and more or less closely affiliated with them are the meetings of nine special societies. The usual entertainments are offered by the citizens of Denver, and excursions of more than usual interest are planned to precede and follow the meeting. The geology, paleontology, flora, archeology and mining resources of the region are of peculiar interest to scientific men, and the scenic beauty of the state and of the surrounding states is known throughout the world.

THE Association is this year particularly fortunate in its retiring and in its incoming presidents. Other eminent men have presided over the Association, but perhaps not before have they united scientific eminence with such great services to the Association and the organization of science in America. Those who have heard or read the presidential addresses which Professor Woodward gave last year before the American Mathematical Society and before the New York Academy of Sciences will look forward with great interest to the Denver address, which we hope to publish in the next issue of this journal. Professor Minot, the incoming president of the Association—whose portrait is given as frontispiece—is known here and abroad for his important contributions to embryology, physiology, animal morphology and zoology. As a boy Minot collected insects, and his earliest publications were on entomological topics. Graduating at the age of twenty from the Massachusetts Institute of Technology in 1872, he could at that time find in America no good opportunity to carry on advanced studies and consequently went abroad and spent three years in Germany and France. He was given the S.D. by Harvard in 1878 and appointed lecturer in the medical school in 1880, being promoted to an assistant professorship in 1887 and to the full professorship of histology and embryology in 1892. At first Minot's work was chiefly physiological—while a student under Ludwig at Leipzig he published an article showing that muscles can maintain their contraction without forming carbonic acid—and in the direction of experimental biology, his investigations covering topics such as growth, heredity and the differentiation of tissues. This work led to two important laws, namely, that, aside from minor fluctuations, the power of growth diminishes from birth onwards, there being really in animals no period of development as opposed to decline; and that the decline in the rate of growth is correlated with the increase and differentiation of the protoplasm of the cells. Another field of early study was the structure of worms. Here his most important result was the demonstration that the Nemertean worms, which had always been classed with the Plathelminths, form a distinct class. The microscopic anatomy of insects and vertebrates was the subject of a number of investigations, among them an extended essay on the histology of the locust, which contains many new observations on insect anatomy.

Owing to the claims of his professorship, strictly embryological work has steadily grown more predominant during the last twenty years. His first important embryological paper was a comparative study of the uterus and placenta, being the first comprehensive account of the microscopic anatomy of the human uterus during pregnancy, and containing many additions to knowledge. During recent years his writings—which in all number over one hundred and fifty titles—have chiefly presented the results of various embryological investigations. The book on 'Human Embryology,' first published in 1892, is a standard work here and in Europe.

As has been indicated, Professor Minot, while making these important contributions to science and conducting a department in a great medical school, has found time to take a leading part in what may be called the organization of science. He has written admirable articles and addresses of a general character, published in this and in other journals; he has by his publications and personal efforts done much to advance medical education and to unite it with biological research; he has accomplished much for bibliography, for the building and equipment of laboratories and in other directions. Following the suggestion of

Professor Hyatt, he founded the American Society of Naturalists and has been its president; he was one of the active founders of the Marine Biological Laboratory at Wood's Holl, and it was chiefly through his exertions that the American Society for Physical Research was started. He has been president of the American Morphological Society, and since 1897 president of the Boston Society of Natural History. He is, of course, a member of the National Academy and of many other scientific societies. In 1885 he was general secretary of the American Association and in 1890 vice-president for the section of biology. The Association is fortunate when the country produces for its presidents such men as Professors Woodward and Minot.

THE BRITISH CONGRESS ON TUBERCULOSIS.

THE recent congress in London was as much an institution for public education as a scientific meeting. As a rule people do not profit greatly by learning about the diseases to which they are subject, but consumption is an exception. This disease is still regarded by many as hereditary and incurable; its existence is consequently ignored and concealed, and becomes a source of danger to others. But consumption is a curable and especially a preventable disease. Post-mortem examinations of those dying by accident show that about one-half of all people living in cities have had tuberculosis of the lungs, usually of course without knowing it. The disease has been cured without precautions and under unhygienic conditions. When detected in time, tuberculosis is not only curable, but is one of the most easily cured of chronic diseases. It has been decreased by one-half in Great Britain by improved sanitary conditions; it has within a few years been decreased by one-third in New York City as the result of municipal control. The disease is chiefly spread by the tubercle germs in the sputum carelessly scattered abroad, and chiefly favored by general insanitary conditions. It is consequently a matter of great concern, both to those who suffer from consumption and to those brought in contact with them,—and practically every one belongs to one of these clases—that the public should be educated to understand and support the measures required to combat the most terrible of all diseases.

If the congress in London accomplished more for the education of the laity than for the increase of knowledge, this is not to be regretted. The reception of foreign delegates, their presentation to the king and elaborate entertainment, led to the wide reporting of the proceedings in the press, and many of the papers were intended for the general public rather than for the specialist. Professor Koch's admirable address, which is published in this issue of the MONTHLY, can be read with interest and profit by any one, though it contains the announcement of important scientific research. Professor Koch's claim that the bovine tubercle cannot develop in the human body naturally attracted much attention, as it is obviously a matter of great practical importance. Lord Kelvin, Professor Virchow and other authorities, however, do not regard Professor Koch's experiments and observations as conclusive. Attention does not seem to have been called at the congress to the important experiments of Dr. Theobald Smith, of Harvard University, published some three years ago in 'The Journal of Experimental Medicine.' These demonstrate the difference between the human and bovine tubercle bacilli.

THE RECENT COMET.

THE first brilliant comet of the century has come and gone. Although at one time so bright that it was visible in daylight, it was seen by few persons and at but two northern observatories. It was discovered, April 24, by Mr.

Halls, of Queenstown, Cape Colony, and was later, but independently, discovered at the Peruvian Station of the Harvard Observatory and elsewhere. Its path lay about 20° south of the sun, so that it was especially well situated for observers in the southern hemisphere. It seems remarkable that so bright a comet could escape more general attention. Bad weather and its southern position in part account for this, but the chief reason is associated with the path of the comet, and the position in which the earth chanced to be at the time. From the interstellar spaces the comet swept into the solar system on the opposite side of the sun from the earth. On this account, doubtless, it was not seen until it had already passed perihelion. At that time it was visible in the morning. A week later it was seen in the evening sky. At one time, except for the inclination of the plane of its orbit to that of the earth, it was moving directly towards us, but, swung about by the sun's attraction, it passed between that luminary and the earth. By the middle of May the comet and the earth were moving in nearly opposite directions. For a large part of the time during which the comet was under observation, it was visible only in strong twilight. About May 5 its position was more favorable, and it was a splendid object. It has now passed out of sight. The comet is described by Mr. Innis, of the Cape Observatory, when first seen, as of a deep yellow color. The nucleus was condensed, and of about the same brightness as Mercury. It had a tail about 10° long, but no coma, or 'hair.' As soon as the comet had emerged from the evening twilight, early in May, its most unique feature became apparent. This was a faint secondary tail, which preceded the comet, as it left the sun, at an angle of about 40° from the primary tail, which had become double. The main tail at this time, according to Mr. Lunt, was about 7° long, while the faint one was three times as long, or about 25°. Between these two were also two other very faint tails. At no time did the comet approach very near to the sun, or to the earth. Good photographs of it were obtained at the Royal Observatory, Cape of Good Hope, and at the Harvard Station in Peru. This adds one more brief but interesting chapter to the history of comets; but in spite of their frequent appearance and the attention which they receive, comets still remain, in many respects, one of the unsolved astronomical puzzels.

VITRIFIED SILICA.

ONE of the most promising of recent developments in connection with chemical and physical apparatus has been the discovery of practicable methods of working vitrified quartz. With all the serviceability of glass and porcelain, there is a real need for some plastic material, more infusible, more insoluble, more fully transparent, more elastic, and more stable under changes of temperature than glass. These needs would be supplied by quartz, were it not for the great difficulty of working it. When touched with the flame, quartz splinters so badly as to be almost unworkable, though in time past a few have used it for small objects, and some ten years ago Professor Boys introduced the use of quartz fibres, which have found several important applications in the physical laboratory. To Professor W. A. Shenstone, however, belongs the credit of having rendered practicable the working of quartz into more or less complicated apparatus. The most important step in his process is the preparation of a non-splintering silica, which he accomplishes by heating quartz in small pieces to a temperature of about 1,000°C. and then throwing it into cold water. The white, enamel-like mass obtained can then be subjected to any changes of temperature without splintering. It is worked in the hottest possible oxy-hydrogen

flame, becoming plastic enough for manipulation only above the melting point of platinum. In preparing tubes the first step is a rod, which is made by fusing small pieces of silica together, one after another. This rough rod is then re-heated and drawn out into finer rods about a millimeter in diameter. These rods are then bound around a thick platinum wire and heated till they adhere to each other forming a tube which can then be drawn out and worked much as a tube of glass. By blowing a small bulb on the end of the tube, surrounding it with a ring of silica, re-heating and blowing, the tube can be lengthened or the bulb enlarged at will. From this starting point it is possible to make quite complicated apparatus.

An examination of vitrified silica reveals several properties which give it a peculiar value for many purposes. Its melting point is so high that a platinum wire imbedded in a thick silica tube can be fused so as to flow out, before the tube softens sufficiently to lose its shape. Its coefficient of expansion is far less than that of any similar substance, being only one-seventeenth that of platinum. This expansion is very regular up to 1,000°, when it diminishes rapidly up to 1,200° and from this point on it contracts. Up to 1,500° it remains practically solid. In its expansion it differs very markedly from quartz, which not only has a much higher coefficient of expansion, but which at 570° expands so rapidly that it is shattered. A rod of vitrified silica can be heated white hot and then immediately plunged into liquid air without suffering injury, indeed it gains in elasticity when thus treated. These properties promise to be of great value in the construction of thermometers. Its transparency to the ultra-violet rays of the spectrum will give it a decided advantage over glass in spectroscopic work. It is also interesting to note that tubes of silica can be heated sufficiently high for nitrogen and oxygen to unite directly on passing through them. It is, on the other hand, slightly permeable to hydrogen at a temperature of 1,000°C. Altogether vitrified silica offers an interesting field of development in the immediate future.

TWO REMARKS CONCERNING THE 'MONTHLY.'

THE following paragraph is reproduced from the 'Electrical World,' as a favorable occasion for two remarks that it has for some time seemed desirable to make:

In the current number of our esteemed contemporary, the POPULAR SCIENCE MONTHLY, which is, alas! more popular than scientific in the single particular that its pages lie largely sealed from mortal eye until separated by that anachronous atrocity —the paperknife—appears a delightful article by Professor J. J. Thomson, 'On Bodies Smaller than Atoms,' an abstract of which appears in the Digest. It is a commentary upon the spread of technical education that a paper on this most abstruse subject, and actually containing some little algebra, should find the light of day in popularized literature, and awake a gleam of recognition from the eyes of many who are not scientists. Twenty years ago such an article on such a subject would have lain on popular benches as caviare to the multitude. It is difficult to say which commands our admiration the more—the article itself, or the fact that the great world should be capable of admitting it into semi-popular literature. Either consideration presents a triumph, the one over inanimate, the other animate, nature.

The first remark concerns trimming the pages of the MONTHLY. It appears from the correspondence of the publication department that to trim or not to trim is a burning question. An 'anachronous atrocity' is pretty strong language, but it seems to define a widespread creed. Some people apparently do not know that there is a good scientific reason for not trimming the edges, namely, that a magazine or book that has been trimmed cannot be properly bound afterwards. Conse-

quently all librarians prefer untrimmed copies, and the publishers of this magazine must provide for some fifteen hundred libraries. Then, we are not prepared to admit that it is unscientific to regard æsthetic considerations. Untrimmed copies look better to most people, and there are a few who even enjoy the use of the paperknife. This preference may be in large measure a survival; still a trimmed magazine seems to be ready for the waste-paper basket, whereas an uncut copy seems to be waiting for its place on the library shelf. Accordingly, copies of this magazine with the edges cut are supplied to the news-stands, but untrimmed copies are mailed to subscribers. Any subscriber, however, who asks for trimmed copies will receive them.

The second remark to which the editorial in the 'Electrical World' gives occasion is more important. It is indeed a matter for congratulation that this country is able to support a journal which calls itself popular and yet publishes only articles strictly scientific in character. Such a journal obviously does not appeal to children or to superficial readers; and its very existence bears witness to the presence in America of a large class of highly educated and thinking people. It may be, however, that some of those who have read the magazine for thirty years regret a certain change in its character and do not appreciate that this is simply an evolution fitting it to existing conditions. Some years ago the truths of evolution needed a fearless advocate, but when these are preached from the pulpit there is no longer need of a special organ. The daily press now publishes articles everywhere of a readable and light character on scientific topics, and no monthly magazine is complete without one or two such articles. What the country needs is a journal that will set a standard of accuracy and weight, and will separate the real advances of science from the vagaries of the charlatan. An article such as Professor Thomson's 'On Bodies Smaller than Atoms' must be read with care, but when understood, it is, as the 'Electrical World' remarks in the editorial from which we have quoted, 'more entertaining than the story of the early crusades and more astounding than those of the Arabian Nights.'

SCIENTIFIC ITEMS.

By the death of Charles Anthony Schott, the government loses one of its most distinguished officers. He was born in Germany, but was for fifty-three years connected with the U. S. Coast and Geodetic Survey. His distinguished position in the scientific world is sufficiently indicated by the fact that the Paris Academy of Sciences made to him the first award of the Wilde prize, which is given without regard to nationality for the most important researches in the physical sciences.—We regret also to record the death of H. W. Harkness, a student of the cryptogams and prominent for his services to science on the Pacific coast, having been for many years president of the California Academy of Sciences; of James Marvin, formerly professor of mathematics and astronomy and chancellor of the University of Kansas; of Williis H. Barris, known for his contributions to paleontology and long president of the Davenport Academy of Sciences; of George K. Lawton, an astronomer of the U. S. Naval Observatory, and of Charles Mohr, a well-known botanist, recently connected with the Geological Survey of Alabama.—Among foreign students of science the following deaths are announced: of Henri de Lacaze-Duthiers, the eminent French zoologist; W. Schur, professor of astronomy at Göttingen; Johannes Lamp, a geodesist of Kiel University; Henri d'Orléans, known for his geographical explorations in Asia and Africa; of C. E. Peek an English meteorologist;

Eleanor A. Ormerod, the English entomologist; and Baron Adolf Erik Nordenskjöld, the Swedish arctic explorer and naturalist.

PROFESSOR RUDOLF VIRCHOW will celebrate his eightieth birthday on October 13; a research fund is being collected in his honor and he has been made a knight of the Prussian order 'pour le mérite,' this mark of imperial favor having been long delayed, apparently owing to his liberal politics.—Professor A. W. Rücker, the physicist, has been elected principal of the newly organized London University.—Two of the prizes created by the will of Alfred Nobel will be awarded to Dr. Niels R. Finsen of Denmark, for discovering the light treatment for lupus, and to Professor I. P. Pavlov, the Russian physiologist, for his researches in nutrition.—Dr. Patrick Manson has been awarded the Stewart prize of the British Medical Association for his researches in pathology and tropical diseases.

SIR JOHN MURRAY has returned from a six months' expedition to Christmas Island, during which he crossed the island from end to end, the first occasion on which it has been traversed.—Prof. Frederick W. Starr, of the University of Chicago, has completed a four months' expedition among the Mexican Indians.—Professor Engler, director of the Botanical Garden at Berlin, has visited the Canary Islands, in order to study their flora.—Professor C. L. Bristol, of New York University, has left New York to direct the Biological Station at Bermuda.

AN international botanical association had its first meeting at Geneva beginning on August 7.—The Fifth International Congress of Criminal Anthropology will be held in Amsterdam from September 9 to 14, 1901.—The summer session of the American Mathematical Society was held at Cornell University, Ithaca, N. Y., during the week beginning on August 19.—The American Forestry Association will hold its meeting in affiliation with the American Association at Denver on August 27, 28 and 29.

ACCORDING to the census taken on March 31, the population of England and Wales was 32,525,716, being an increase of 12.15 per cent. in ten years. The increase in the preceding decennium was 11.65. The pereentage increase of London was only 7.3 per cent., its population now being 4,536,034. There has, however, been a large increase in the surrounding country, the population of Middlesex having nearly doubled. The population of Ireland is 4,456,546 and of Scotland 4,471,957. The change in the population of Ireland and of Scotland in the past sixty years is remarkable:

Year.	Ireland.	Scotland.
1841	8,197,000	2,620,000
1851	6,574,271	2,888,742
1861	5,798,967	3,062,294
1871	5,412,377	3,360,018
1881	5,174,836	3,735,573
1891	4,704,750	4,025,647
1901	4,456,546	4,471,957